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BULLETIN OF THE HISTORY OF MEDICINE

ORGAN OF THE AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE
AND THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

Founded by HENRY E. SIGERIST

Editor—OWSEI TEMKIN

VOLUME XXIV JULY–AUGUST, 1950 NUMBER 4

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Editor—OWSEI TEMKIN

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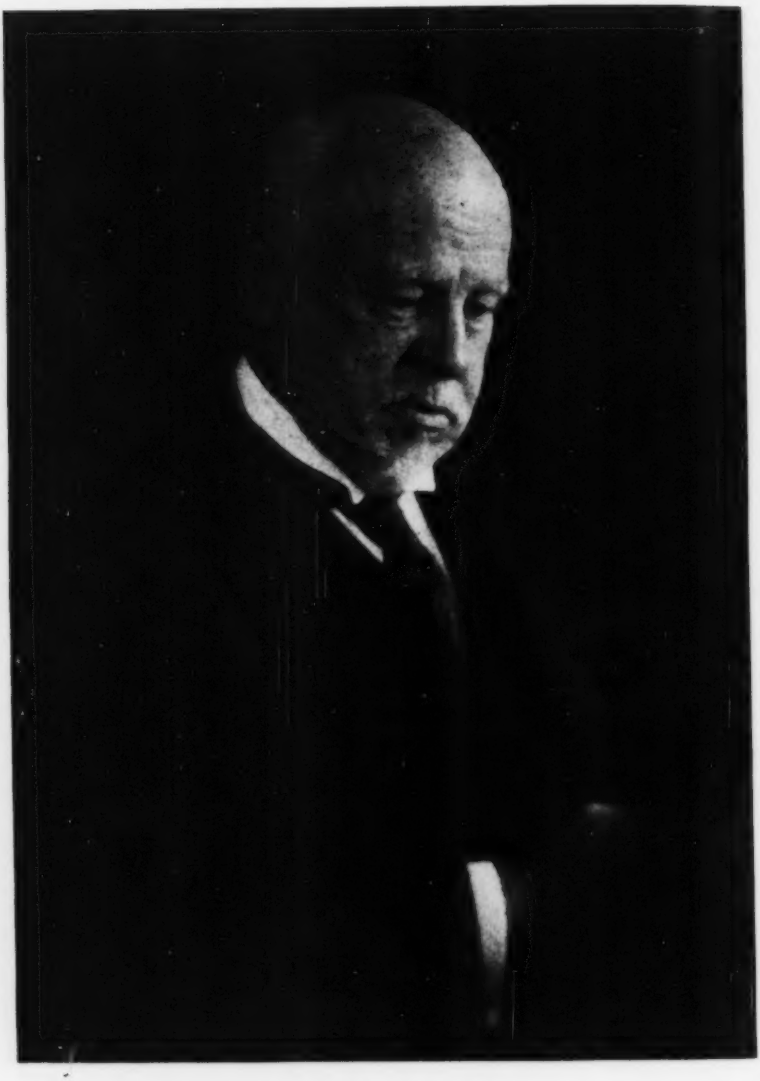

WILLIAM H. WELCH

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EDITORIAL

Dr. WILLIAM H. WELCH, April 8, 1850—April 30, 1934

Dr. Welch liked to speculate on the names of those men who influenced decisively the course of medicine through the centuries. If we were to draw up such a list today, we should hardly fail to include his own name. There have been great doctors whose influence on the art of medicine showed itself more directly and brilliantly than that of Dr. Welch who was not a clinician. In the field of medical science, others have outranked him in the number and importance of discoveries. But if we ask for the names of those individuals who helped to elevate American medicine to the rank it holds in the world today, then the name of Dr. Welch will perhaps be the very first to come to our minds. Fate was certainly kind to him; it granted him a long life during a period of American history when his particular gifts could exert themselves most fruitfully in many directions. But while his stimulating thought and organizational abilities made themselves felt even beyond the confines of the United States, the development of the Johns Hopkins Medical School and the Johns Hopkins Hospital is inseparably bound to his name. It was, therefore, fitting that these two institutions combined to sponsor a celebration of the centenary of the birth of Dr. Welch.

The celebration took place on Saturday, April 15, 1950. In the after-

noon a meeting was held in the Hurd Memorial Hall of the Hospital, with the following program: *

Presiding: Dr. Detlev W. Bronk, *President, The Johns Hopkins University*

Dr. Welch's influence on the development of pathology: Dr. Ernest W. Goodpasture, *Vanderbilt University*

Dr. Welch's influence on public health: Dr. Allen W. Freeman, *The Johns Hopkins University*

Dr. Welch's influence on the history of medicine: Dr. Richard H. Shryock, *The Johns Hopkins University*

Dr. Welch's influence on medical education: Dr. Alan Gregg, *Director of the Medical Sciences, The Rockefeller Foundation*

Reminiscences of the early days of the medical school: Sound motion picture of Dr. Welch made in 1932.

These exercises were followed by an invitation tea at the Welch Medical Library where the Librarian, Dr. Sanford V. Larkey, had arranged an exhibit of published as well as unpublished material bearing on Dr. Welch's life and work. The exhibit will be on display until this fall.

The celebration was concluded with a dinner at the Sheraton Belvedere Hotel at which many institutions were represented by their delegates, and Abraham Flexner, A. B., LL. D., Johns Hopkins, delivered the after-dinner address.*

Dr. Welch closed his active career at the Johns Hopkins University by organizing the Institute of the History of Medicine and becoming the first occupant of the newly established chair in this discipline. Years ago the American Association of the History of Medicine had decided to honor Dr. Welch's name by the establishment of a William H. Welch medal "for particular contributions of outstanding scholarly merit in the field of medical history." However, it was only in 1949 that, through a generous gift by Mr. Joseph Fabian Ford, the medal could be established. The first award of the medal was therefore made this year, coincident with Dr. Welch's hundredth birthday. Dr. Henry E. Sigerist, the first successor

* The papers presented at this meeting, together with Mr. Flexner's address, will be published as a supplement to the *Bulletin of the Johns Hopkins Hospital.* By an editorial arrangement, Dr. Shryock's address, which was also delivered at the Boston meeting of the American Association of the History of Medicine, appears in that supplement as well as in the present issue of the *Bulletin of the History of Medicine.*

of Dr. Welch at the Johns Hopkins Institute, was selected as the recipient, and an announcement was accordingly made at the twenty-third annual meeting of the Association at Boston, May 21-23, 1950.

At this same meeting, the Association devoted a special session on the evening of May 21 to a Welch Centenary Celebration. Upon a suggestion of Dr. Viets, President of the Association, the speakers for this session had been selected from the Johns Hopkins University. Dr. Viets was in the chair and the program listed:

The European background of the young Doctor Welch
Owsei Temkin

Doctor Welch's interest in bacteriology and the related biological science
Barnett Cohen

Doctor Welch's pursuit of medical history
Richard H. Shryock

Demonstration of the Welch sound motion picture.

The three papers delivered at the Boston meeting are published in this issue of the *Bulletin* in the order of their presentation. There has been added an English translation by Mrs. C. L. Temkin of the " Reminiscences of the Summer Semester, 1877, at Breslau " which C. J. Salomonsen, the old friend of Dr. Welch, had brought out in 1914. This article had originally been dedicated to Paul Ehrlich on the occasion of his sixtieth birthday. However, it constitutes such an important and well recognized source for Dr. Welch's biography that it seemed appropriate to have a full translation prepared and included in the *Bulletin's* contribution to the memory of Dr. Welch.

Owsei Temkin

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THE EUROPEAN BACKGROUND OF THE YOUNG DR. WELCH

OWSEI TEMKIN

It was in Europe, in Bad Homburg, in the fall of 1927, that I first remember meeting Dr. Welch at a gathering of German medical historians. This acquaintance was renewed in the Leipzig Institute of the History of Medicine. I offer these personal remarks because they may explain why I wished to speak on the *European* background of the young Dr. Welch. His biography by Simon and James Flexner,[1] based as it is on an extensive study of all personal documents, leaves little space for the discovery of new biographical data. But it occurred to me that some of the facts set forth by the Flexners might gain in perspective if supplemented by German sources. These latter might offer an additional commentary on the two years, from 1876 to 1878, that Dr. Welch spent in Germany and with which alone my remarks will be concerned.

Dr. Welch sailed for Europe on April 19, 1876.[2] Personal inclinations, the influence of various associates, and general currents of the time had determined his goal and aims. Pathological anatomy had been cultivated by his teachers at the College of Physicians and Surgeons: Alonzo Clark, Henry Berton Sands, Charles McBurney and, above all, Francis Delafield with whom he worked in pathology after his graduation.[3] He wished to become a professor of pathological anatomy, if possible at the Johns Hopkins University, and his name had already been recommended to President Gilman.[4] But it was clear to him that, as yet, he was unprepared for such a position. He possessed a microscope, yet did not know how to use it.[5] He had to study histology and the place to do this was Germany. His own teachers, it is true, had been reared in the French tradition.[6] But the pilgrimage of American doctors to Germany for postgraduate work had already begun. Abraham Jacobi directed his "attention especially to the

[1] Simon Flexner and James Thomas Flexner, *William Henry Welch and the heroic age of American medicine*, New York, The Viking Press, 1941.

[2] *Ibid.*, p. 76.

[3] *Ibid.*, p. 63 ff.

[4] *Ibid.*, p. 71 ff.

[5] *Ibid.*, p. 64.

[6] According to Walter B. James' article in Kelly and Burrage, *Dictionary of American medical biography*, New York-London, Appleton, 1928, p. 315 f., Francis Delafield "had acquired in Germany a conviction of the overwhelming importance of practical studies in pathological anatomy." I have been unable to obtain details about Delafield's studies in Germany.

great position of German medical science in the world." [7] Wood, among others, told him to study microscopy in Germany.[8] Of course, there was no certainty of his receiving the Hopkins position, and things being what they were, medical practice might turn out to be a necessary alternative. But even in this case, " absorbing a little German lore " would put him in a more favorable position.[9] Thus Dr. Welch set out with the hope to become a competent pathologist or at least to perfect himself in some clinical knowledge, preferably neurology in which his teacher Seguin, himself a pupil of Charcot,[10] had interested him.

On Dr. Welch's own testimony it was Seguin who directed him to Strasbourg,[11] which had passed from French into German hands but five years before. The Germans were determined to wean the Alsatians from their attachment to France and, to further this purpose, they had decided to create a model university in Strasbourg, appointing some of the best men as professors. Although they failed in their major aim, they had succeeded in establishing an excellent new university. Especially in pathology and histology, the fields of Dr. Welch's immediate studies, conditions were almost ideal. Von Recklinghausen, the pathologist, was one of Virchow's most outstanding pupils. Waldeyer was not only a brilliant anatomist, but before accepting the chair of normal anatomy in Strasbourg, had taught pathology in Breslau.[12] Moreover, these two men were on such good terms that they wished their institutes to be housed in the same building.[13] Thus the transition from von Recklinghausen's institute, where Dr. Welch was studying gross pathology, to Waldeyer's course in normal histology must have been easy enough. Nor was Waldeyer unaccustomed to American students. In his autobiography he gives the names of three Americans who had worked with him during his Strasbourg period. They were: George Livingston Peabody, Landon Rives Longworth, and A. J. Lanterman.[14] Dr. Welch's name, as you see, is not

[7] Flexner and Flexner, *op. cit.*, p. 70.
[8] *Ibid.*, p. 74.
[9] *Ibid.*, p. 76.
[10] Walter L. Burrage in Kelly and Burrage, *op. cit.*, p. 1088.
[11] Flexner and Flexner, *op. cit.*, p. 78.
[12] Wilhelm von Waldeyer-Hartz, *Lebenserinnerungen*, Bonn, Friedrich Cohen, 1921, p. 128.
[13] *Ibid.*, p. 150.
[14] *Ibid.*, p. 159: " Aus den Vereinigten Staaten Nordamerikas arbeiteten bei mir Peabody, Longworth und Lanterman, der die nach ihm benannten kerbförmigen Bildungen der markhaltigen Nervenfasern im Strassburger anatomischen Laboratorium entdeckte, die gleichzeitig und völlig unabhängig von Schmidt in New-Orleans aufgefunden wurden."

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among them. This, however, is understandable, since he merely participated in a regular course, whereas Longworth and Lanterman, at least, had done actual research work. Longworth's article, "Ueber die Endkolben der Conjunctiva," appeared in the *Archiv für mikroskopische Anatomie* in 1875, although the work had apparently been done in 1874.[15] It deals with the existence of terminal bodies of sensitive nerves in the conjunctiva. Incidentally, the same volume contains another anatomical work by an American, W. T. Alexander from Boston, also from the Strasbourg anatomical institute, entitled "Bemerkungen über die Nerven der Dura mater" (Remarks on the nerves of the dura mater).[16] Whether Waldeyer failed to mention Alexander because this paper had not been prepared under his guidance, or whether he had forgotten Alexander, I am unable to decide. Lanterman's preliminary notice on the minute structure of mylienated nerve fibres is dated Strasbourg, August, 1874.[17] He must, therefore, have been at work at approximately the same time as Longworth. Lanterman, contemporaneously with, but independently from H. D. Schmidt in New Orleans, described the clefts which are still known under the name of Schmidt-Lanterman clefts or incisures.

Now all three of these American pupils of Waldeyer had one thing in common: they had graduated from the College of Physicians and Surgeons a few years before Dr. Welch.[18] Under these circumstances I wonder whether his going to Strasbourg was quite the "leap in the dark" he represented it to be to his sister.[19]

[15] L. R. Longworth (Cincinnati, Ohio), Ueber die Endkolben der Conjunctiva, *Archiv für mikroskopische Anatomie*, 11. Bd., 1875, pp. 653-660. Some editorial remarks made by Waldeyer suggest that the work had been done about a year before (see p. 657) and that the manuscript had been completed a considerable time before its publication (see p. 659). Besides, according to his biography in Kelly-Burrage, *op. cit.*, p. 757, Longworth returned home in the fall of 1874.

[16] W. T. Alexander aus Boston, Bemerkungen über die Nerven der Dura mater, *ibid.*, pp. 231-234.

[17] A. J. Lanterman (Cleveland), Bemerkungen über den feineren Bau der markhaltigen Nervenfasern. Vorläufige Mittheilung. *Centralblatt für die medicinischen Wissenschaften*, vol. 12, 1874, pp. 706-709. The more detailed publication with illustrations appeared in the *Archiv für mikroskopische Anatomie*, vol. 13, 1877, pp. 1-8.

[18] For Longworth and Peabody see Kelley-Burrage, *op. cit.*, pp. 757 and 951. Polk's *Medical and surgical directory of the United States*, 1886, p. 182, lists A. J. Lanterman in Buena Vista, Colorado, as having been graduated by the College of Physicians and Surgeons in 1868.

[19] Flexner and Flexner, *op. cit.*, p. 78. William T. Councilman, in turn, during his postgraduate studies in Europe, 1880-1883, went to von Recklinghausen in Strasbourg and to Cohnheim who then held a chair in Leipzig. He was to become Dr. Welch's first associate in 1884 in Baltimore. See Alan M. Chesney, *The Johns Hopkins Hospital and*

In Strasbourg, Welch had devoted himself chiefly to attending laboratory courses: a demonstration course in gross pathology with von Recklinghausen, normal histology with Waldeyer, and physiological chemistry with Hoppe-Seyler.[20] The next stage, Leipzig, marked his acquaintance with the German methods of actual research. He now took up pathological histology with Wagner who assigned to him a case of lympho-sarcoma for investigation, and, more important perhaps, he stumbled into Ludwig's physiological institute. I use the word "stumbled," because Welch would later have it that he had really gone to Leipzig in order to attend Heubner's course on nervous diseases and that Heubner's shift from psychiatry to pediatrics had left him with free time which led him to Ludwig.[21] The Flexners have pointed out that in his letters home Welch did not mention Heubner at all and that this is all the more remarkable since he would hardly have failed to acquaint his father, who was somewhat impatient of his son's theoretical bent, with the practical nature of his intended study. Apparently Heubner, in 1876, was still giving his course on psychiatry and Welch attended it. But even though the lectures were delivered, they failed to arouse enthusiasm.[22] For Heubner himself writes in his autobiography that he had no liking for the matter, that he was repelled by the hopelessness of psychiatric therapy, and that he refused to consider an associate professorship in psychiatry on which he was sounded out by the Leipzig medical faculty in 1877.[23] Thus, whether from an excess of free time or disappointment over Heubner's course, Dr. Welch found himself in Ludwig's laboratory. In Carl Ludwig, Dr. Welch discovered a man who lived for research and who taught his advanced students the spirit in which it had to be pursued. Ludwig put Dr. Welch on some microscopic investigation of "the nervous apparatus of the heart."[24] Although his results were not published, the work had the much greater importance of admitting Welch into the community of active scientific investigators. The impression Ludwig made on Welch is, perhaps, best exemplified by a state-

the Johns Hopkins University School of Medicine, vol. 1, Baltimore, The Johns Hopkins Press, 1943, p. 90 f. It would also be interesting to know who the other Americans were to whom Welch alludes as fellow students in Strasbourg (see Flexner and Flexner, *op. cit.*, p. 80 f.).

[20] For this and the following see Flexner and Flexner, *op. cit.*, pp. 78-84.

[21] *Ibid.*, p. 84.

[22] *Ibid.*, p. 474, and Simon Flexner, William Henry Welch, a biographical sketch, *Science*, n. s., vol. 52, 1920, pp. 417-433, see p. 420.

[23] Wolfgang Heubner (ed.), *Otto Heubners Lebenschronik*, Berlin, Julius Springer, 1927, p. 103 f.

[24] Flexner and Flexner, *op. cit.*, p. 86.

ment of February 1877 when he referred to Ludwig and Claude Bernard as "undoubtedly the two greatest living physiologists."[25] As far as Ludwig is concerned, this statement is not without interest and you will permit me a personal comment in this connection. As a medical student at the University of Leipzig, I attended courses in the physiological institute which had once been directed by Ludwig. Some of Ludwig's original apparatuses were still standing around and there was even an "Institutsgehilfe" who could look back to the days of Ludwig. The memory of the master was very much with us; we were very conscious of the fact that one of the most outstanding German physiologists had taught in this same place. Nevertheless, I wonder whether we should unhesitatingly have named Ludwig as *the* greatest German physiologist of the later 19th century. In our minds, his name would have competed with those of Du Bois Reymond and Helmholtz. But there is no doubt that Ludwig was the most outstanding teacher, having an especially high number of foreign pupils,[26] and I think that this has given Ludwig an even greater reputation abroad than he possessed at home.

The year 1877 was to become one of the most decisive years in Dr. Welch's life. The work with Wagner, Ludwig, and the zoologist Leuckart, left its stamp not only on his appreciation of scientific research but also on his whole philosophical outlook. Besides, he met John Shaw Billings and spent an evening with him in Auerbach's Keller. This meeting made Welch decide upon another year in Germany and also led to a strengthening of the ties with the Johns Hopkins University. Finally, Ludwig and his assistant, Kronecker, inveighed upon him to turn to Breslau and continue his pathological studies with Julius Cohnheim.[27]

In the sound motion picture which is to conclude this session you will hear Dr. Welch refer to the beautiful summer of 1877 in Breslau. He will mention to you Salomonsen's article in the *Berliner klinische Wochenschrift* of March 16, 1914, which gives reminiscences of that time.[28]

This article of Salomonsen's is indeed so vivid in its description and so important for the association of Cohn, Cohnheim, Weigert, Ehrlich, Heidenhain, Koch, Lassar, and others that Dr. Welch, himself, in later

[25] *Ibid.*, p. 85.

[26] See also George Rosen, Carl Ludwig and his American students, *Bull. Hist. Med.*, vol. 4, 1936, 609-650.

[27] For this paragraph see Flexner and Flexner, *op. cit.*, pp. 86094.

[28] Carl Jul. Salomonsen, Lebenserinnerungen aus dem Breslauer Sommer-Semester 1877, *Berl. Klin. Wchschr.*, vol. 51, 1914, pp. 485-490. This article, was also used by Flexner and Flexner, *op. cit.*, and Heymann (see footnote 37). The English translation by Mrs C. L. Temkin appears below, pp. 333-351.

years, consulted it to refresh his memory.[29] In 1931, when he once more visited Europe, Dr. Welch was asked to speak on his reminiscences of Robert Koch before the medical society of Berlin. In his diary he mentions his attempts at getting hold of Salomonsen's "book" — Sudhoff finally succeeded in tracing the article for him. Welch read it and entered an extract in his diary, probably for further reference. At the same time he added some reminiscences of his own. Thus he writes that Cohnheim, at that period, was "little interested in mere pathological histology, rather scornful of pretty stainings.—'Was malen Sie heute, Herr Welch'[30] he would say in looking at my haematoxylin stained sections. Weigert scolded me for bringing haematoxylin into the microscopical course and impressing Cohnheim with its value—said I had made trouble for the Assistants as C(ohnheim) wanted them to use haematoxylin in these student courses."

To understand this remark it is necessary to realize, as Salomonsen informs us, that Cohnheim was chiefly interested in experimental work so that his assistant, Carl Weigert, was the real anatomist in the institute.[31] Weigert and even more so the "master dyer,"[32] Ehrlich, still a medical student, were of course pioneers in the use of new staining methods in histology and bacteriology. But whatever benefit Dr. Welch may have derived from his association with Weigert—it was Cohnheim with whom he had come to study, and it was in line with Cohnheim's investigations in pathological physiology that he undertook and finished his important research on pulmonary edema.

We may here raise the question whether Dr. Welch can be considered a pupil of Cohnheim in his general approach towards pathology. Cohnheim is remembered as the representative of pathological physiology. This is not to be understood as if Virchow and his school lacked interest in the experimental approach or in any way underrated the significance of the physiological interpretation of pathological phenomena. Nor does it mean that Cohnheim, himself a pupil of Virchow's, underrated the importance of morphological studies, both macroscopic and microscopic. The

[29] The information in this paragraph is derived from a diary (box 209, diary no. 37) of Dr. Welch's which Dr. Sanford V. Larkey kindly made available to me. It is part of the Welch material deposited in the Welch Medical Library of the Johns Hopkins University.

[30] "What are you painting today, Mr. Welch." It is not customary in Germany to use the title of "doctor" in addressing one's medical colleagues.

[31] See the English translation of Salomonsen's article by C. Lilian Temkin, below, p. 336.

[32] "Hauptfärber," see *ibid.*

difference, such as it existed, was rather one of emphasis upon what constituted general pathology. Virchow's concepts of general pathology had emerged in close association with his fundamental work on cellular pathology.[33] To some extent, at least, cellular pathology was to replace the older medical systems. Whether or not it is justifiable to consider cellular pathology a system too, one cannot quite escape the impression in reading Cohnheim's " Introduction " to his *Lectures on General Pathology* and his inaugural address of 1878 " On the tasks of pathological anatomy," that he, just as Ludwig many years before,[34] tried to avoid any overemphasis of the cellular theory. " Far be it from me to deny our indebtedness to these manifold [pathological] systems for many a great and important advance in pathology; but they have as such, whatever be the names they bear, a purely historical interest. General pathology knows no other direction and no other classification than that which obtains in physiology, and following this science, we shall in succession treat of the pathology of the circulation, digestion, respiration, nutrition, etc." [35] Now I think that Welch succeeded in avoiding a onesided attachment to pathological anatomy or physiology. Though decidedly influenced by both Ludwig and Cohnheim, he returned, after the summer in Breslau and some weeks in Vienna, to Strasbourg and to von Recklinghausen,[36] Virchow's staunch pupil. Nor would onesidedness have suited the American scene where Welch was to perform his work. Thorough post-mortem dissection, microscopic technique, and pathological experiment had all to be naturalized: there was no room for cultivation of one at the expense of the others.

I cannot take leave of that summer in Breslau without commenting on Dr. Welch's relationship to bacteriology, in whose history a new chapter was just being written. As you know, Robert Koch, then residing in Wollstein, had demonstrated his epoch-making experiments on the anthrax bacillus to the botanist Cohn in Breslau in the spring of 1876, almost one year before Welch's arrival in the city. Cohnheim was called in and

[33] This is especially evident in *Die Vorlesungen Rudolf Virchows über allgemeine pathologische Anatomie*, Jena, Gustav Fischer, 1930.

[34] Ludwig's lack of enthusiasm for cellular pathology has been noted by Flexner, *Science*, n. s., vol. 52, 1920, p. 422. I have tried to sketch Ludwig's attitude in "Metaphors of human biology," *Science and civilization*, ed. Robert C. Stauffer, Madison, University of Wisconsin Press, 1949, p. 173.

[35] Julius Cohnheim, *Lectures on general pathology*, vol. 1, London, The New Sydenham Society, 1889, p. 11.

[36] Flexner and Flexner, *op. cit.*, p. 107 f.

Weigert too witnessed the demonstration.[37] From then on there was close contact between Koch and the Breslau workers. Of interest to us is Koch's short visit to Breslau on June 6, 1877.[38] This I believe to be the event to which Dr. Welch refers in his diary:[39] "It was on the occasion of one of these visits that he [i. e. Koch] came one day to Cohnheim's laboratory and spent an hour or more with Cohnheim—Afterward Cohnheim introduced me to Koch in the laboratory, and was full of enthusiasm about his discovery. At this time Koch's special interest was in the reproduction of bacteria—fresh and stained—by microphotography." I mention this entry because there is some uncertainty about the date as well as the nature of this early encounter with Koch.[40] And here we are confronted by a somewhat puzzling problem. In 1877, Welch met the leaders in German bacteriology: Cohn, Koch, Klebs (whom he visited in Prague), Weigert; his intimate friend, Salomonsen, was a pioneer in the new science; his teacher Cohnheim had expressed enthusiasm over Koch's work. Yet all this made little impression upon Welch. On his return stay in Strasbourg, von Recklinghausen pointed out the significance of bacteria to him, but when Welch passed through Paris on his way home, he apparently had no thought of visiting Pasteur.[41] To be sure, Welch was busy with his experiments, but he had time to attend other lectures, and at any rate it is his lack of interest and appreciation that strikes us. Perhaps we come a little nearer to the solution if we consider the peculiar position of bacteriology in 1877. There were still a number of men who denied to bacteria any decisive etiological rôle. For instance Fischer, the Breslau surgeon, had not yet espoused Lister's antiseptic treatment.[42] However, the group

[37] Bruno Heymann, *Robert Koch*, vol. 1, Leipzig, Akademische Verlagsgesellschaft, 1932, p. 150.

[38] This date is established by the note in Cohn's diary: "Dr. Koch besuchte am 30.6. das Institut" (Heymann, *op. cit.*, p. 195).

[39] See above, note 29.

[40] Flexner and Flexner, *op. cit.*, p. 100 f., believe the meeting between Koch and Welch to have taken place some time between October 15 and 18, when Koch demonstrated his work an anthrax to the London physiologist, Burdon Sanderson (Heymann, *op. cit.*, pp. 210-212). By that time, however, Salomonsen had already left Breslau, so that his own reminiscences (see this *Bulletin*, p. 334) must refer to June 6. Besides, he mentions that Koch paid a short visit to Cohnheim's institute which well agrees with Dr. Welch's remark. Since Welch in his own letters home apparently did not mention Koch at all and, in later years, does not seem to have been certain of the details of this encounter, the question of the date remains somewhat doubtful.

[41] Flexner and Flexner, *op. cit.*, pp. 101 and 109.

[42] Salomonsen, this *Bulletin*, p. 345. Fischer's conversion to antisepsis must have taken place soon afterwards; see *Festschrift zur Feier des hundertjährigen Bestehens der Universität Breslau*, ed. G. Kaufmann, 2. Teil, Breslau, 1911, p. 302.

of radical sceptics was fast shrinking. The idea that microorganisms could and did cause diseases was being widely recognized. But the question as to what diseases exactly were of bacterial origin was far from solved and the uncertainty proved a disturbing element. In a paper of 1877, Carl Weigert defined his own position as follows:[43]

I believe that in some cases (recurrent fever and anthrax) the action of bacteria has been proved with such certainty as is attainable for scientific investigations, that in a series of other cases this action can be assumed as probable but that in a great many cases it cannot yet be assumed with any certainty.

And Cohnheim in the above mentioned address in 1878 expressed himself in a somewhat similar vein:[44]

For the whole very important group of infectious diseases, the *Contagium animatum* of earlier authors is at present no longer a hypothesis and science today quite rightly demands of the pathologist that in each infectious disease he find the parasitic organisms to which it owes its origin. The only point where unity has not yet been reached is the question as to how far the territory of infectious disease should be staked: there are authors who attribute an infectious character even to the malignant tumors, and there are others who do not hesitate to refer every so-called cold, every catarrh, to an infection.

Taking these two statements by adherents of the bacteriological school together, we see that only for relapsing fever and anthrax was a bacterial etiology accepted as strictly proved, that the infectious nature of even a common cold was still open to doubt and that the demonstration of pathogenic microorganisms for individual diseases was a research problem. In other words, in 1877, bacteriology was not yet a field with well established methods which could be learned and taught. It was not yet a domain of those pathological sciences which Dr. Welch had come to make his own. Even the German pathologists who, in principle, recognized the rôle bacteria played, held somewhat aloof from the development of bacteriology.[45] Relatively few of them took an active interest in the matter. With Koch's appointment to Berlin, bacteriology became combined with hygiene and even men like von Recklinghausen, originally enthusiastic over the new insight, distanced themselves from a science which they began to consider as a competitor.[46] All this has to be remembered if we ask for the

[43] Carl Weigert, *Gesammelte Abhandlungen*, vol. 2, Berlin, 1906, p. 426.

[44] Julius Cohnheim, *Gesammelte Abhandlungen*, Berlin, 1885, p. 615.

[45] Address by Dr. William H. Welch on the history of pathology. *Bull. Hist. Med.*, vol. 3, 1935, p. 13. Even the surgeon Theodor Billroth in a letter of August 9, 1877, wrote despairingly about the difficulties of obtaining positive proof of bacterial etiology; see Heymann, *op. cit.*, p. 200.

[46] B. Naunyn, *Erinnerungen Gedanken und Meinungen*, München, Bergmann, 1925, pp. 370 and 431. Otto Lubarsch, *Ein bewegtes Gelehrtenleben*, Berlin, Julius Springer, 1931, p. 25.

reasons why Dr. Welch showed such slight response to bacteriology during his first European journey. It was only during his subsequent years in New York that bacteriology developed to an extent which made its study imperative.[47] Dr. Welch's second trip, in 1884-85, culminating in a course with Koch in Berlin, was undertaken for this purpose.[48] That Welch's previous failure to be impressed by bacteriology can be explained by the requirements of American medical science as it then existed is further documented by the similar experiences of Welch's main competitor, T. Mitchell Prudden. For Prudden, too, studied pathology in Germany and he too had to go back in order to attend school again with Robert Koch.[49]

The few remarks offered may have shown that Dr. Welch's assimilation of German science was partly determined by the conditions of that science in 1876-78. Partly it was determined by his own sense of what would be needed in the United States, especially with regard to the study and teaching of medicine. I have stressed the early, though loose, ties that bound Dr. Welch to the future of Johns Hopkins. I should supplement this by pointing out that Dr. Welch's interest was not limited to medical science but extended to medical education as well. In January 1877, two months after the meeting with Billings, he remarked: "I think that the mainspring of German thoroughness lies in their preliminary education. No man can study medicine or any profession who has not gone through a course of study at least equal to a college course at Yale and Harvard." [50] Impressions like these fitted very well into the ideas concerning premedical education voiced by Billings and President Gilman.[51] But having said all this, I have to add that Dr. Welch did not accept everything he found in Germany. From the picture Salomonsen and the Flexners have given us, we have reason to assume that Dr. Welch met his German colleagues with an open mind, ready to learn and to accept what impressed him as good. He seems to have entered with ease into friendly relations with his professors and fellow students. But he was not attracted by the more clamorous and overbearing aspects of German student life. In the beginning of this paper, I said that I met Dr. Welch in Germany in 1927. At that time, he impressed me by the patience with which he listened even to the youngest speaker and by his dignified yet friendly and utterly unassuming manner. I think he listened rather than talked. And I like to believe that

[47] Flexner, *Science*, n. s., vol. 52, 1920, p. 429.
[48] Flexner and Flexner, *op. cit.*, p. 137 f.
[49] *Biographical sketches and letters of T. Mitchell Prudden, M. D.*, Yale University Press, 1927, p. 52.
[50] Flexner and Flexner, *op. cit.*, p. 91. [51] Chesney, *op. cit.*, p. 45 ff.

when he was in Germany some 50 years earlier, his attitude had been fundamentally the same. It was one of the remarkable things about Dr. Welch that he was wise in his youth, yet remained young in his older years. This may, perhaps, allow us to draw a parallel in spite of the lapse of years. Although Europe, and especially Germany, exerted a great influence upon the young Dr. Welch, they only formed a background for his development. He was always conscious that his roots and his future lay across the sea, in America.

COMMENTS ON THE RELATION OF DR. WELCH TO THE RISE OF MICROBIOLOGY IN AMERICA

BARNETT COHEN

I have had occasion, recently, to explore the relations of Dr. William Henry Welch to bacteriology during the period of his active interest in this rapidly growing field of knowledge.[1] Several of his published addresses and his technical writings provide some information, but there seems to be nothing in print that presents a sufficiently clear picture of this portion of his many-sided career.

The general view seems to be that Dr. Welch pursued bacteriology as a valuable adjunct to his major laboratory interest, pathology, that his own outstanding technical contributions to bacteriology itself were comparatively few, and that this science was not very significantly influenced by his activities. However, it is possible to entertain a qualification of the last opinion in the light of the evidence to be presented here briefly. These comments may be best described as footnotes to one phase of Dr. Welch's extraordinarily fruitful life.

The investigations conducted in his laboratory at Baltimore were both in bacteriology and in pathology, combined in varying proportions. His assistants and students were encouraged to select and work on problems of their own choice; and the spirit of the environment was conducive to excellence in research. Among the studies carried out were those dealing with various infectious diseases and the microbes associated with them. Dr. Welch's published investigations on hog cholera, lobar pneumonia, diphtheria and typhoid fever, on infections by the colon and the gas bacillus (the "Welch bacillus"), and on wound and surgical infections indicate the general range of his own bacteriological activities. Participating in some of these studies were his assistants, Alexander C. Abbott, Simon Flexner and George H. F. Nuttall. It is of interest that about half of these publications on bacteriological topics appeared within the four-year period 1889-1892; most of the remainder appeared intermittently through 1900. The total output from associates and students in his laboratory was of course greater in volume and flowed continuously through the years.

Early in his career, Dr. Welch perceived the practical advantages to be

[1] This account is based largely upon material in the collection of the Welch Medical Library, The Johns Hopkins University and in the Archives of the Society of American Bacteriologists; and upon Dr. Welch's "Papers and Addresses," 1920.

gained from unhampered research in the medical sciences, and he persuasively applied his unique talents to the encouragement and ultimate general acceptance of this viewpoint in American medicine. His consistency of purpose is attested by the affiliations that he maintained with professional scientific societies up to about the turn of the century,—that is, through the period of his active career in the laboratory. These affiliations (excluding those with medical societies) were few but significant. He presided over the organization of the Laboratory Section of the American Public Health Association; he helped to found the Society of American Bacteriologists; he was one of the founders of the American Association of Pathologists and Bacteriologists; and he participated in organizing the Section of Physiology and Experimental Medicine of the American Association for the Advancement of Science. This small list is instructive in its implications.

The fields represented (physiology, bacteriology, pathology, experimental medicine) were those in which he had had particular interest and experience. Moreover, they were integral parts of a broad concept as expressed in his remark (1906): "All of the medical sciences are interdependent, but each has its problems and methods, and each is most fruitfully cultivated for its own sake by those specially trained for the work." It is a fact, however, that in 1894 he considered it "better to place bacteriology with pathology or hygiene than to make of it a separate department." He preferred to teach bacteriology in conjunction with pathology in the early days of the infant science, and this pattern persisted in his department for many years,—a curious contradiction of his professed general concept.

In the main, bacteriology in America followed that in Europe. It seems, however, that we possessed at least two or three practically independent investigators in this field. The first was Thomas J. Burrill (1839-1916) of Illinois who studied molds and bacteria from 1870-1892, and who in 1878 conclusively demonstrated the bacterial origin of pear blight. From this discovery rose the science of plant pathology, said to be the only science which has had its inception in this country. The second was George M. Sternberg (1838-1915), later Surgeon-General of the U. S. Army, who while stationed at an army post in Walla Walla, Washington Territory, studied disinfection along with his duties as surgeon of the post. Later he was transferred to Baltimore for a while and continued his studies on bacteria at the laboratories of H. Newell Martin and Wm. H. Welch at the Johns Hopkins University. A possible third was Greene Vardiman Black (1836-1915), physician-dentist, an unusually

versatile naturalist and inventor living in the Chicago region. He published in 1884 a book entitled: *Formation of Poisons by Micro-organisms*, —an attempt at the description of the nature and properties of the poisons produced by microbes.

There were a number of later home-grown bacteriologists, such as Theobald Smith (1859-1934) in Boston, Erwin F. Smith (1854-1927) at the Department of Agriculture in Washington, and William T. Sedgwick (1855-1921) in Boston; but these were more nearly contemporary with Dr. Welch, Dr. F. G. Novy (Ann Arbor), and the others who came back with the new knowledge and techinques learned from the European masters. The Cartwright Lectures of the College of Physicians and Surgeons, New York, on "The Relations of Microorganisms to Disease" given in 1883 by William T. Belfield (1856-1929) of Chicago (one of Koch's students) were probably the first given in this country on medical bacteriology as we understand it. It was toward the middle and late 1880's that centers of study in bacteriology sprang up in medical schools and agricultural colleges in various parts of the country and the science began to receive more general cultivation.

The encouragement of Dr. Welch's natural interest in experimental medicine was provided by his exposure, on his first two trips to Germany (1876 and 1885), to the emerging progressiveness and vitality of German medical science, and particularly by his associations with such great leaders as Carl Ludwig, Julius Cohnheim, Carl Weigert, and Robert Koch. From these associations stems Dr. Welch's emphasis on physiology and on bacterial infection as two cardinal factors in the study of pathology. Through his example and teaching, both became powerful instruments in the transformation of pathology and medicine from static to dynamic sciences in this country.

It was natural, therefore, that he should affiliate himself with or help to organize in this country professional societies or groups in physiology and bacteriology. There is no record that he ever joined the American Physiological Society (founded in 1887 through the initiative of S. Weir Mitchell, Henry Bowditch and Newell Martin). However, William H. Howell states that Dr. Welch "had the highest opinion of the Physiological Society as a means for the promotion of scientific medicine in this country. It was of great assistance to the society to have the cooperation of three such eminent medical men as Welch, Osler and Mitchell." There can be hardly any doubt that Dr. Welch helped to create and channel the interest and enthusiasm that led to the founding (in 1902) of the new section of

Physiology and Experimental Medicine in the American Association for the Advancement of Science. On this occasion, his underlying view was expressed in the following terms. "For the success of the section, it seems to me most desirable to secure, so far as possible, the interest and support of the existing national societies which represent the fundamental medical sciences, as anatomy, physiology, pathology, bacteriology and hygiene."

Under the auspices of the American Public Health Association, a committee had operated since 1894 to formulate standard methods for the chemical analysis of water. This Laboratory Committee developed, in October, 1899, into the Section of Bacteriology and Chemistry (now the Laboratory Section of the A. P. H. A.). Dr. Welch presided at this formal transformation, which incorporated bacteriology as a necessary discipline in the practice of hygiene and sanitation.

Another, more pregnant, idea was converted into reality when, two months later, the Society of American Bacteriologists was organized through the efforts of Alexander C. Abbott, Edwin O. Jordan, and H. W. Conn. Dr. Welch was invited to preside at the organization, but modestly deferred to others, giving as an excuse his recent officiation at the Laboratory Section. At the same time, he asked to be counted in "for whatever is agreed on at the meeting regarding the new organization." He thus became a charter member, along with such leaders as J. G. Adami, Ludvig Hektoen, Veranus A. Moore, W. H. Park, T. M. Prudden, W. T. Sedgwick, and Theobald Smith, of the first independent organization in the world dedicated specifically to the promotion and service of bacteriology as a science.

Dr. Welch's connection with the Society of American Bacteriologists was far from passive. He served as their second president in 1901; and he participated in formulating the basic proposition upon which the society has flourished, namely that microbiology represents a fundamental discipline and a liberal science. The record shows that he also participated actively in the scientific discussions at their meetings through 1907. This was some time after the pressure of other interests had caused him to relinquish active work in the laboratory, but not active concern in the promotion of the laboratory sciences.

We come now to an admirable facet of Dr. Welch's character. The young bacteriological society had among its members a number of pathologists, some of whom conceived its scope to be too restrictive. Led by the dynamic Dr. Harold C. Ernst of Boston, they organized in 1901 the

American Association of Pathologists and Bacteriologists. Dr. Welch, naturally, became a charter member of this group (and later its president), because it represented a closer approach to his professional predilections. Nevertheless, he remained true to the principle that each of the sciences is most fruitfully cultivated for its own sake by those specially trained for the work. So far as I can determine, he cultivated both of the societies with his customary urbanity and wisdom.

Dr. Welch was an adept in the bacteriological knowledge and practice of his day. He had a very extensive grasp of the literature, the biological significance of the phenomena, and their impending developments, scientific and practical. For one who so successfully taught the lesson of bacteriology in the broadest sense to medical science, he seems to have acted curiously in that at least one of his major assistants was apparently discouraged from taking his course in bacteriology. Simon Flexner wrote: "[He] did not permit me to take the regular course in bacteriology. . . . Therefore such bacteriology as I acquired was done piecemeal, which is a strange accident, because in the long run I paid more attention to bacteriology and its outgrowths than I did to pathology in the conventional sense." Later, in speaking to a medical student contemplating preparation in bacteriology, Dr. Welch advised medical training as a background and pathology in particular as very important for the better understanding of the problems to be solved. He spoke of the opportunities for research both in practice and in the laboratory, and urged an interest in some basic science, and later in some problem.

This attitude with respect to training in bacteriology in those days was, obviously, based on the practical needs of the medical scientist and on the facilities available to meet them. There were few, if any, adequate centers in which the science was cultivated for its own sake. Let it be remembered that bacteriology had been developing as an applied science, without much of a prior store of purely scientific knowledge; and the practice of bacteriology was conducted mainly in connection with departments of hygiene or of pathology. Finally, since the methods of the young science were rather primitive and not especially difficult, an apt student could master them sufficiently in a short time and turn to one of the many problems awaiting investigation.

It is unnecessary to review here Dr. Welch's important labors toward the improvement of medical education in this country, except to point out that, by helping to create the climate favorable and necessary for the growth of the experimental medical sciences, he contributed greatly to the advance-

ment of microbiology. He successfully expounded the unity of purpose of the medical sciences, and the value of scientific investigation for its own sake and for its healthy effect upon teaching. His influential voice emphasized repeatedly—in quarters where it would be most effective—the vital necessity of laboratories for the advancement of medicine and hygiene through bacteriology and the other sciences. Dr. Welch gave direction and force to a great movement for scientific progress in this country.

One of the outstanding fruits of this movement was the rise of microbiology. This science has expanded greatly in range and depth during the past sixty or more years, and its future appears limitless. Its beginnings in this country were stimulated by a relative handful of far-seeing pioneers among whom was Dr. Welch. Their students were the intellectual seeds for the new growth, the development of which undoubtedly was facilitated through his influence. He would doubtless disclaim this as one of his contributions to microbiology. Nevertheless, he was preeminently influential in obtaining, in this country, the wide establishment of laboratories in which the science had the opportunity to grow to its present respected position among the natural sciences. Equal in importance were the contributions that he made as a great teacher and as a participant with the other leaders in the founding, and establishment of the fundamental scope, of the first organization in the world dedicated to the service and advancement of microbiology as a science.

DR. WELCH AND MEDICAL HISTORY

RICHARD HARRISON SHRYOCK

Dr. Welch accomplished so much in scientific and professional spheres that his work in medical history has naturally been viewed as tangential to his main career. One might therefore expect to find that his historical interests were of an incidental nature, but nothing could be further from the truth. All the evidence indicates that his delight in historical studies began with his college years and continued into his last days here at the Hopkins Hospital. Pushed aside for considerable periods by professional activities, this interest reasserted itself again and again; and his final appointment as professor of medical history was a more logical one than he himself—with his usual modesty—was willing to admit. It was logical not only in view of a long concern with the subject but also because Welch was an able medical historian in his own right. No American of his time displayed more insight into the development of certain modern aspects of medicine. It is extraordinary that this should have been the case with a man who, of necessity, gave the greater part of his years to non-historical activities.

Welch was first attracted to history as an undergraduate at Yale, where he aspired to become a professor of the classics. He subsequently studied in Germany under men who had some respect for historical backgrounds, but there is no record of any one teacher who inspired his enthusiasm. He attended Haeser's lectures on medical history at Breslau and found them "deadly dull"; but this experience did not discourage him. His interest in the past seems to have been self-sustaining, and he continued to read general historical works throughout his life. Upon becoming immersed in medicine, he brought to bear upon this field the historical perspective which was already essential to his whole outlook.

After Welch came to the Hopkins in 1885, he was too busy for several years to give much heed to medical history. Dr. Billings had urged a decade before that the subject should be taught here, and did lecture on it himself for a short time. Under these circumstances, it is strange that Dr. Welch made no mention of the matter in outlining plans for the medical school in 1886. Two years later, however, he advocated in an address at Yale: "the study of the history of medicine, a subject which notwithstanding its interest and values is much neglected. Nothing," he went on, "is more liberalizing and conducive to medical culture than to follow the

evolution of medical knowledge."[1] He thus anticipated, almost at the beginning of his Hopkins career, the program to which he finally gave all his energies some thirty years thereafter.

During the next year, 1889, Osler and Kelly joined the Hopkins group, and Welch found them kindred spirits in historical as well as other matters. Osler led in founding the historical Club at the Hospital, of which Welch became the first president. At the initial meeting, the latter spoke on the value of historical studies to the physician; and thereafter he continued these talks to the Club year after year until he took over the direction of the School of Hygiene in 1918. In arousing the interest of his hearers, Welch had the advantage of his unique reputation and personality: whatever the master found of value, the students viewed with respect. It is improbable that any specialist in medical history, however able, could have elicited the same response. Rufus Cole noted that the club meetings "exerted a deep and lasting influence on all the students and assistants. They stirred into life a new side of our personalities, the cultivation of which is important, even in those whose chief interest is science. . . ."[2]

In a word, Welch breathed the spirit of the humanities into medicine—a living spirit, which animated current medical concepts and problems and which was never deadened by a mere curiosity about the past for its own sake. Although he spoke at first of cultural values, the historical approach to medicine became more than this to him—it was a means by which the scientific navigator could take his bearings from the past and so chart his course into the future. He supported all his favorite programs, such as the need for integrating medicine with other sciences, by evidence drawn from past experience.[3] In his treatment, history was related to and threw light upon the present. It had, in an ultimate sense, utilitarian as well as cultural values. This outlook placed Welch in line with the utilitarian or pragmatic attitudes, which had long characterized American society.[4] History was a serious business.

Between 1918 and 1925, Welch was absorbed in the development of the School of Hygiene. Upon reaching his seventy-fifth year on the latter date, he expressed a natural desire to retire; but his many friends had no in-

[1] Simon Flexner and James Flexner, *William Henry Welch and the Heroic Age of American Medicine* (N. Y., Viking, 1941), 421.

[2] *Ibid.*, 422 f.

[3] See his "Unity of the Medical Sciences," "Interdependence of Medicine and the Other Sciences of Nature," given in *Papers and Addresses*, III (Baltimore, Johns Hopkins Press, 1920), 305 ff. and 315 ff.

[4] "Consideration of the Introduction of Surgical Anaesthesia," *ibid.*, 229.

tention of permitting this. Seeking some further opportunity for him and knowing his long interest in the subject, several of them conspired with Abraham Flexner to secure an endowed chair for him in medical history. Just because he took this field seriously, Welch was disturbed to learn of expectations that he would now "loll about" in the proposed chair during his "loafing years." He at first declined the suggestion, on the ground that to take it "at my age would merely emphasize the spirit of dilettantism in which the subject is regarded and generally pursued in this country." The very fact that Welch recognized this danger was, of course, the best evidence that he would not succumb to it. Convinced, finally, that the post would be offered to no one else, he accepted in 1926; and the General Education Board thereupon provided the necessary funds in connection with the endowment of the Medical Library.[5]

And so, at 76, Welch embarked upon his third major professional program with youthful enthusiasm. He decided at once that the professorship must be expanded into an institute in association with the library. Knowing German institutions well, his mind naturally turned to the famed Institute of the History of Medicine at Leipzig as a model. Hence, just as he had gone to Germany for advanced medical training over fifty years before, he now returned there in 1927 for further light on his new profession.

His journals for this trip are replete with human details which make interesting reading. Here was a man of almost eighty undertaking alone an arduous journey, on which he was not only to explore the possibilities of an institute but also to select and purchase $5,000 worth of books for the Medical Library. Yet Welch took it all as a matter of course. He had his little difficulties because of absent-mindedness; as when he forgot to declare at customs his beloved cigars, and officials held up the boat train at Southampton because of their suspicions of this venerable smuggler.[6] In London he was much impressed by the Wellcome Historical Medical Museum, and its potentialities for the teaching of medical history. Arriving subsequently at Leipzig, he was much pleased by the Institute there. Dr. Sudhoff, the founder, and his successor, Dr. Sigerist, extended every hospitality; and Welch found them congenial and stimulating. In long talks with Sudhoff, he learned much about plans for a new institute at Berlin which was to pursue the history of the physical sciences as well as of medicine. He also pumped Sudhoff on the reputation of various

[5] Flexners, *Welch*, 418.
[6] Diaries, 1927 (no. 24, "A"), 27 ff.

German medical historians, and pondered the chances of persuading one or more of them to come to Baltimore.[7]

Meantime, he secured many suggestions concerning the book lists he was compiling. Sigerist had explained that he was interested in good editions and standard files rather than in first editions and rare books as such and Welch agreed entirely with this attitude. Unlike Osler, he was not essentially a bibliophile; he wanted a working library. For several months in 1927 he bought nothing, discussing desirable purchases with Arnold Klebs at Nyon as well as with many others. In the fall, however, he arranged an extension of his leave; and after a vacation in Italy, began buying in earnest. Since he kept no systematic records of purchases, he was confused by his own lists and fearful of duplications; but he enjoyed the search for all that. He never knew just how much he had spent, and soon had exceeded the sum authorized; but noted that he would gladly go into his own pocket to make up any difference.[8] This whole time-consuming task was by no means the least of his services to the later Hopkins Institute and to the Welch Library.

The possibility of an institute at Baltimore meanwhile hung in the balance. In Paris during the preceding summer, Welch had found both Flexner and Vincent encouraging about the prospects of an endowment; but further uncertainty ensued when there was a reorganization of the Rockefeller boards. Welch returned to New York in September, 1928, and in the following February made his first official address as the incumbent of the new chair. Upon his recommendation, Dr. Garrison was offered and accepted the librarianship of the Welch Library; and this best-known of American medical historians thus became potentially available for an institute staff. Welch also brought in as an assistant Dr. Stephen D'Irsay, whom he had met abroad during the preceding year. In October, 1929, the Welch Library and the Department of the History of Medicine were formally dedicated; and in May of the next year, the General Education Board extended an endowment for the Institute.

Welch had now achieved all that he had hoped for, and plunged into arrangements for organizing the Institute and developing its activities. Dr. John R. Oliver was added to the staff, and Dr. Charles Singer of London gave the first Noguchi lectures—a series endowed by Dr. Emanuel Libman of New York. Formal instruction began at the Institute during the fall of 1930, in its quarters on the upper floor of the Medical Library. Welch, Garrison, Oliver, and D'Irsay all offered courses; but few stu-

[7] *Ibid.*, p. 77 ff.

[8] *Ibid.*, No. 29, pp. 91 ff.

dents attended and Welch confessed that the faculty was "rather overdoing it." Welch realized from the start that his energies were not adequate for the difficult task of carrying the Institute beyond its actual establishment. He therefore finally and irrevocably resigned on July 1, 1931, and the directorship was offered to Dr. Harvey Cushing and subsequently to Dr. Charles Singer of London. Both of these gentlemen eventually declined. In the fall of that year, the post was vacant. Dr. Sigerist was visiting lecturer at the time, and his success was so instantaneous that he was offered the directorship and accepted this in the spring of '32.

The subsequent history of the Institute is well known. It has remained a unique institution in this country and is one of many monuments to Dr. Welch's achievements. Although he constantly gave Osler credit for arousing interest in medical history at Hopkins, Welch alone envisaged the possibilities of an institute devoted to the field. And he alone—both because of his interests and his reputation—could have established it.

Welch, despite his broad grasp on medical history, felt some diffidence upon acquiring the status of a professional. When he attended an International Congress of Medical History at Leiden in 1927, he was welcomed with enthusiasm; but observed that he "had no right to claim admission to such a gathering of Fach-historiker." On another occasion he declared that he was "throwing off temporarily my disguise as a medical historian." He viewed himself as a beginner in his 70's. Although he enjoyed the thought that "For a budding historian, I have a running start," he declared that he would now have to go "to school again."

The Flexners, in their able biography, suggest that in this respect Welch was simply adhering to his usual, realistic evaluation of his own work.[9] But modesty was also a characteristic of the man, and I really believe that here he underestimated himself. Welch was a competent medical historian long before he accepted a chair in the field. Judged objectively by published writings, his work stands out from that of most contemporary Americans in more than one respect.

In the first place, his interest was always in the broad sweep of scientific and professional trends, rather than in things curious or episodical. At the same time, he realized the significance of minor as well as of great figures; and the importance of basing general narratives on an understand-

[9] Flexners, *Welch*, 424.

ing of local developments. In a word, he was at all times the historian as distinct from the antiquarian.[10]

Moreover, the same breadth of interest which transcended antiquarian details opened to him vistas of the larger relationships of medical to general history. In this regard, Welch was quite unusual among American contemporaries, most of whom wrote medical history largely in isolation from its social and intellectual backgrounds. "I think," he declared, "there is scarcely any subject which, for the study of the history of medicine, does not possess significance; the history of medicine requires information on all the conditions of civilization of the particular period under consideration. How can one grasp its real significance without knowing the state of contemporary knowledge of all departments of science and philosophy?"[11] In his actual writings, Welch gave chief attention to the interrelationships between medicine and the physical sciences; and took less heed of the impact of social forces upon medicine. But the larger concept was clearly there, nourished no doubt by his reading in general history.

This being the case, Welch decided that—as professor of medical history—he should establish contacts with both general historians and those devoted to the history of science. So far as I remember, no other American physician interested in medical history had made any such effort up to that time. Welch attended the annual meetings of the American Historical Association at Minneapolis in 1931, and I recall his sitting at the speaker's table on one occasion with unassuming dignity. He doubtless went out to see what could be learned from these professionals.

I suspect it never occurred to him that they in turn could have learned much from *him*. For most general historians had not—and have not yet—effectively incorporated the history of science into their synthetic narratives. There is, for example, hardly a college text in American history which provides any adequate notion of the role played by science in molding our present civilization. This should be of concern to scientists as well as to historians, for the public in consequence has little understanding of what science is all about. Welch was that rare person who knew both his science *and* his history; and had he been younger he might have done much to enlighten the general historians.

This observation may seem a bit sweeping, but I make it deliberately

[10] *Cf.* George Rosen, "Levels of Integration in Medical Historiography," *J. Hist. Med and Allied Sc.*, IV (Autumn, 1949), 460 ff.

[11] Flexners, *Welch*, 435.

after a re-examination of his published papers. For Welch had a flair for getting at the essence of an historical trend, and then explaining it in a clear and incisive manner. He wrote well, as he spoke well, and always with apparent ease. It is true that he usually omitted the critical apparatus typical of contemporary historians—citations, bibliographies, and the like—because he wrote from a well-stocked memory and with the aid of reference and other general works which were at hand. He rarely had time to dig into the original sources. In this respect he suggests the literary historians of the mid-nineteenth century, rather than the "scientific" historians of his own day.

Yet he was quite capable of operating in the most orthodox professional manner, as is illustrated by his address on the "Relation of Yale to Medicine." He found it difficult at first to see much relationship between Yale and medicine, up to 1901 when the address was given; but just because of this it seemed necessary to consult the sources in detail. The result was an informative paper which is quite stimulating at times, as in its new evaluation of Nathan Smith; and one lays it down with a changed perspective on the Yale story. This was only one of many occasions in which Welch displayed an encyclopedic knowledge of men and events in American medicine—data which he always related to meaningful trends and backgrounds. In any case, this particular essay was buttressed by such detailed notes and citations as would have warmed the heart of the most meticulous scholar.[12]

Welch stimulated his hearers with fluent summaries and interpretations, pointed usually toward their present implications. But he was also effective in analyzing issues and in raising critical questions. In a paper on "The Evolution of Modern Scientific Laboratories," for example, he proposed a whole series of queries which needed further investigation.[13] One wishes that he could have conducted an historical seminar.

That he would have been sympathetic with graduate students, yet not uncritical withal, goes without saying. His kindly interest in younger men, and the affectionate esteem in which they held him, were proverbial. If chance had thrown him more into the way of historical students, he would certainly have been as stimulating to them as he was to junior medical men. I know whereof I speak for—if you will pardon a personal reference—Welch went out of his way to encourage my own interest in medical history at a time when such moral support made all the difference

[12] "Relation of Yale to Medicine," *Papers and Addresses*, III, pp. 243 ff.
[13] *Ibid.*, 200 f.

in a beginner's aspirations. One felt, after such encouragement, as if an accolade had been bestowed—and a devout wish to be worthy of it. The man who could thus inspire younger men had the attributes of a great teacher.

What finer tribute can we render to Dr. Welch's memory, in conclusion, than to recall again the circumstances of his final contribution to medical history in this country? At an age well beyond that at which most leaders have retired, he applied to the planning of the Institute the wisdom of a half-century of scientific and professional achievement. In so doing, he raised the standards of the discipline in this country and provided it with an impetus to which it still responds. What would he not have accomplished, had he focused his interests on this field at a more youthful period—say, perhaps, when he was a mere stripling of 60?

REMINISCENCES OF THE SUMMER SEMESTER, 1877, AT BRESLAU

BY

CARL JULIUS SALOMONSEN *

TRANSLATED, WITH NOTES AND A BIOGRAPHICAL INDEX, BY

C. LILIAN TEMKIN

Dear Ehrlich,

I have nothing scientific to offer that would be worthy of your great name, and yet I should like to figure among your admirers in this number dedicated to your honor. For this reason, I am sending some Reminiscences of the Summer Semester of 1877. On the basis of old letters I have called to mind the time when we first met in Julius Cohnheim's laboratory and, with Weigert, Neisser, Lichtheim, Rosenbach, Welch, Lassar, Senftleben, Unverricht, and many others, crowded around the professorial chair of the young Breslau master. As pictures from the workshop of one of the greatest German pathologists, these notes may perhaps be of interest to wider circles; actually, however, they are meant only to waft a gentle breeze from our far-off, happy youth over your mossy, laurel-crowned head.

Your faithful C. J. S.

Copenhagen, February 1914.

In the summer of 1871 I had passed my State Board Examination and in 1876 had made my debut as an author with a short treatise on "The Isolation of Different Forms of Bacteria" [1] in the *Botan. Ztg.* In this treatise I described a method of raising pure cultures that enabled me to set up a collection of pure cultures which, in that early period of medical bacteriology, was, as far as I know, the largest in existence. At last, in the spring of 1877, I passed the doctoral examinations; that means, in German, I received permission to teach as a "Privatdozent." It was now my intention to study pathological anatomy and experimental pathology for a semester abroad. I was not for a moment in doubt as to where I wanted to go. The brilliant work of Julius Cohnheim towered in my opinion so high above all other publications in experimental pathology of the time

* *Berliner klinische Wochenschrift*, vol. 51, I, no. 11, March 16, 1914, pp. 485-490. This whole issue was dedicated to the 60th birthdays of Paul Ehrlich and Emil von Behring.

[1] C. J. Salomonsen, Zur Isolation differenter Bakterienformen, *Botanische Zeitung*, 1876, vol. 39, pp. 609-622.

that I chose Breslau. There I also had the opportunity of closer acquaintance with Ferdinand Cohn. He had become the sought-after adviser of all medical men working in bacteriology because of his diligent and sober studies of bacteria. So, soon after my appointment, I went to Breslau where I arrived on April 2, paid calls on Cohnheim and Cohn, and was received by both of them in a friendly, almost cordial, manner. Next day I went to the pathological institute; a tall, handsome, majestic concierge, Höhndorf, showed me the work table reserved for me in front of a window through which I had a view of the not exactly limpid waves of the Oder. "So that I might enjoy the sea air," said Cohnheim.

It was vacation time. But I had been informed by letter that I need not hesitate to come, as the daily dissections had to be continued throughout the vacation because of the institute's connection with All Saints' Hospital. And, indeed, they went quietly on under the direction of the second assistant, Oscar Lassar; the first assistant, Weigert, was away on a trip to Italy. Soon for me, too, work was going full swing. I began by practising all the famous Cohnheim frog experiments on inflammation, stasis, embolism, etc. Cohnheim himself came very often, though not daily, to the laboratory. Thanks to a visitor of note, I happened to see him in action as an experimenter soon after my arrival. In the hotel where I lived for a few days before finding private rooms in the Lessingstrasse, there sat next to me at the dining table an elderly man with whom I soon entered into conversation. Bartels from Kiel. He had come to see the well-known Cohnheim-Lichtheim edema experiments performed by the authors themselves; his request was fulfilled and the old nephrologist was not a little surprised when he was shown absolutely dry subcutaneous tissues of the animals in spite of the excessive hydremic plethora.

Here I must also mention another guest whose visit primarily concerned Ferd. Cohn, but who also looked up Cohnheim in the institute: Robert Koch from Wollstein, who had already gained a world-wide reputation with his experiments on anthrax, published during the previous year. This time he came with a whole series of microphotographic negatives. Glowing with enthusiasm for the photography of stained bacteria, he spoke of all other methods of reproduction as of something completely obsolete. I was invited to dinner [2] with him by Ferd. Cohn; the only guests were Koch, Eidam, and myself; at table, the conversation was to all practical purposes of nothing but bacteria. Later the whole company went to the photographer who, in the course of the day, had produced

[2] "Zu Mittag"—Germans dine in the middle of the day.

the beautiful positives which were later published in Cohn's *Beiträge*.[8] Koch's visit was of very short duration, but we immediately entered into friendly relations as "bacteriologists," a fact which was of course of the greatest importance for my later studies.

In spite of the intense and rapid development of medical bacteriology in those years, there were, nevertheless, not too many real "bacteriologists" in the stricter sense; while, for instance, Klebs in Prague had a splendid and magnificent bacteriological set-up at his disposal in his institute, there was no experimental work with bacteria at all in the Cohnheim laboratory. When it happened that some anthrax organisms were sent to the institute and Lassar and I were eagerly at work getting a few particularly fine preparations containing rods ready to show to the professor, one of those present remarked: "You know, the professor doesn't much like people to work with such virulent things in the laboratory." In this connection I may perhaps add another small experience in order to show how little acquainted one was at that time with bacteriological technique in pathological circles. When one day I visited Virchow in Berlin in order to deliver a greeting from Panum, he kindly asked about my studies; I answered that I was working particularly in bacteriology. He then made a few remarks about the difficulties of pure culture and added with a smile: "Brefeld once told me that to proceed with complete certainty one should really have a special little laboratory for each species!"

As I said, I was fortunate enough immediately to enter into closer contact with Koch, thanks to bacteriology. I still possess a letter which he sent me in 1879 from Wollstein, with a series of microphotographs, among which were the first successful recurrent preparations, which he had just finished making. And I was one of the first foreigners to whom he granted admission to his laboratory in the Imperial Ministry of Health in the Luisenstrasse, and this at a time when it consisted of one large room with three windows; before each window a work-table; Koch sat in the middle, Gaffky and Loeffler on each side, separated from the master only by two screens. I often visited him later, too, in Berlin. One day he took leave of me in a half ceremonious fashion; it was at the beginning of 1899; I had gone to him with my assistant, now director of the Danish State Serum Institute, Thorvald Madsen, whom I wished to introduce to him. Koch showed us many beautiful malaria preparations; when I

[8] See R. Koch, Verfahren zur Untersuchung, zum Konservieren und Photographieren der Bakterien, *Cohns Beiträge zur Biologie der Pflanzen*, 1877, Bd. 2, Heft 3 (reprinted in *Gesammelte Werke von Robert Koch*, vol. 1, Leipzig, 1912, pp. 27-50, plates 2 and 3).

was about to leave, he presented me with the proofs of his as yet unpublished "Travel Reports"[4] to read on the train and said: "And let's shake hands and perhaps for the last time! I am soon going to the tropics; my trip is so planned that I always arrive at the various places at the most dangerous time of the year. Perhaps we shall not see each other again!" Fortunately he did come back alive and we met many more times, once in Copenhagen and, for the last time, not long before his death, in his beautiful home in Berlin.

With the month of April, vacations were at an end; "official" life in the laboratory started. At the same time there came two men to the institute who became of the greatest importance for my life and studies: Carl Weigert and William Welch.

Weigert, Cohnheim's beloved and prized first assistant, came back from his holidays. For his arrival, Lassar had put garlands on both his microscopes and decked his chair with wreaths of flowers; on the window he had printed a large "Welcome" with picric acid, fuchsin and many other aniline dyes. Under Weigert's direction, you know, microscopic staining was carried on in the Breslau institute with particular fanaticism and the senior workers in the laboratory spoke always with a certain pride of the beautiful "Breslau preparations." Only Cohnheim himself observed the great technical progress connected with the tremendous development in the manufacture of aniline dyes with somewhat greater equanimity: "Ah! So you're painting again!" "But you are really a master dyer!" he sometimes said ironically.

The real "master dyer" in the laboratory was a blond, curly-haired youth of only 22 years, Paul Ehrlich, who always worked with a nervous zeal. Many years ago, when Ehrlich's star was rising rapidly, an old Breslau comrade once said to me in Berlin: "Do you remember Ehrlich as a student in Breslau? We laughed at him because he was always running around among us with blue, yellow, red and green fingers—but he was right after all." Certainly he was right! Perhaps even then the proud and fruitful thought of solving the greatest puzzles and the most difficult problems of biology by means of the ingenious application of dyes was germinating in his young brain; and what did he not later illuminate, interpret, invent, and create with the chemistry of dyes as a starting point!

Weigert was the real pathological anatomist of the institute, prosector

[4] Robert Koch, *Reise-Berichte über Rinderpest, Bubonenpest in Indien und Afrika, Tsetse-oder Surrakrankheit, Texasfieber, tropische Malaria, Schwarzwasserfieber*, Berlin, 1898 (*Gesammelte Werke*, vol. 2, pp. 688-742).

as well as microscopist. Cohnheim himself was tired of the prosectorship; he had become the complete experimenter. He only entered the dissection room when all post-mortems were finished; then he selected something for his courses; only once, on the first day after the vacations, did we have the opportunity of admiring the elegance and skill with which he dissected and taught at the same time; otherwise he willingly left both post-mortem dissections and microscopic investigations to Weigert who was his equal as a prosector and, as a microscopist, perhaps his superior—anyhow more "modern." Zealous, enthusiastic, helpful, and selfless was Weigert as teacher and man, but he was badly treated by earthly and heavenly authorities; when, at last, after many disappointments and much trouble, he had established himself in a pleasant position in the Senkenberg Institute and the German and non-German medical world was preparing to pay homage to him on his sixtieth birthday with the publication of his collected works and the making of a plaque, etc.—he suddenly died shortly before his day of honor.

As I mentioned before, William Welch of New York came at the same time to the institute. He had studied for a semester under Waldeyer in Strassburg and later for eight months under Ludwig. We had many points of contact: like myself, he was the son of a physician; like myself, he hoped immediately upon his return home to be appointed prosector in a large communal hospital; like myself, he considered it self-evident that a budding pathologist should go to Cohnheim's institute. And in 1877 we were actually proud of being the only non-German medical men to seek out Cohnheim. As foreigners we became close friends, were together from early morning till late afternoon, had dinner [5] together every day; and at the same time we were treated in the kindest way by all our German colleagues—spoilt, I should say—and received into their circle with colleague-like friendship. I felt then, and have always felt since, that it was very fortunate that I happened to find as a fellow student in Breslau such a distinguished person and investigator as Welch. Cohnheim knew the value of Welch. He did his part to obtain for Welch the professorship in pathology at the Johns Hopkins University, where he exercised a decisive and beneficial influence on the development of all medicine in the United States, and where he is honored as a patriarch by the present generation of American pathologists.

Oscar Lassar, the second assistant, was a very versatile person—perhaps too versatile. As a youth he had won the iron cross for his bravery in

[5] "Zu Mittag": cf. footnote 2.

the Franco-Prussian War; later he was assistant under Meissner; for some time he had been active in a psychiatric clinic; he was a short story writer, had journalistic interests and sent home correspondence from Roumania and Constantinople; he was a born administrator and organizer, which he later proved on numberless occasions—as general secretary of the international congress of physicians in Berlin, as founder of the famous great dermatological private hospital, and in establishing the public baths. Here in Breslau, Welch and I attended a few very instructive courses in experimental pathology arranged by him; he took the kindest interest in us and was always ready to give us help and advice. When, a few years ago, he was accidentally killed after becoming one of the busiest dermatologists in Germany, I lost a faithful friend whose house in Berlin was always open to me.

Of the regular frequenters of the laboratory I name, furthermore, Ludwig Lichtheim, later for a long time in the Königsberg chair, an ornament to German clinical medicine. Lichtheim was a worker of the first order. He directed the medical policlinic, was directing physician in a hospital for the indigent, was at the same time a busy practising physician, active as " Privatdozent," and worked daily in the institute.

Albert Neisser, too, was a constant visitor. I was somewhat less close to him at that time and had no impression either of his great talent and colossal capacity for work which enabled him later to accomplish such great things in many aspects of his specialty, or of his many artistic interests which made it possible for him, together with his high-spirited, kind and talented wife, to form a wonderful social center in Breslau.

Senftleben, a handsome, elegant surgeon-major, Weigert's bosom friend and comrade in arms in 1870, was Cohnheim's favorite. At that time he was working on keratitis. He was a virtuoso with the microtome and in Cohnheim's gold-staining of the cornea, and he was just about to complete his beautiful piece of work on patterns of regeneration after cauterization with zinc chloride.

Finally, Ottomar Rosenbach, a nephew of Ludwig Traube. He was really there against his will; he wanted to obtain permission to teach at the University of Jena. Nothnagel supported him zealously but there was so much intrigue against him that he withdrew and settled for the time being in Breslau where, under Cohnheim's direction, he carried on his experiments on traumatic causation of valvular lesions. The beautiful results of this series of investigations are general knowledge. Rosenbach was a lively, intellectually dynamic person who dearly loved paradoxes,

and there are many of them in his book, "Physician versus Bacteriologist"[6] of 1903. As a therapeutist, he was no less apt to push his scepticism to the paradoxical. When, one day, he was making fun of the people who prescribe opiates as anti-diarrhetics, someone asked him: "Don't you believe in purgatives either?" "Well, now," he answered, "purgatives are something I am more inclined to believe in!"

In this good company Welch and I went to work under Cohnheim's direction. We followed his courses regularly, the somewhat elementary histology course as well as the masterly, fascinating demonstrations. If, as I mentioned before, he took the actual dissecting somewhat lightly, all the more did he shine as originator and director of all the experimental work in the institute. For this reason we too were gradually completely absorbed in the pathological experiments. At the beginning of June, I wrote to my parents: "Cohnheim and Lichtheim today made a short excursion to procure a horse for edema experiments; so Welch and I more or less had a holiday, i. e. we dissected and watched dissections. Unfortunately, we only rarely manage this when our Lord and Master is here to experiment."

Cohnheim proposed to me two pieces of work which attracted me very little: first of all, I was to reveal the etiology and pathogenesis of valvular lesions by means of histological investigations. This problem I dropped immediately because to me it seemed simply unsolvable. The second, too, the genesis of petecchia, explained by dissection material and experiments on the living frog, I had no great desire to tackle. In contrast, I seized with great joy upon the proposal to study the development of tuberculosis of the eye after inoculation into the camera anterior. At first it went quite badly. The dissection material continually yielded suppurative panophthalmias, and operative material, extirpated lymph glands with "local tuberculosis," was slow to arrive. In order to obtain fresh tuberculous material, Cohnheim inoculated two guinea pigs intraperitoneally with human tuberculosis. When the animals were dissected some weeks later, they showed the most splendid general inoculation tuberculosis! To Welch and me this was a new picture and we were delighted with the beautiful preparation. "As a pathologist one should really say of this, as of Naples: Vedere é poi morir,"[7] remarked Welch—and Cohnheim laughed heartily over his youthful joy and excitement. At last a few

[6] Ottomar Rosenbach, *Arzt contra Bakteriologe*, Berlin, 1903; an English translation by Achilles Rose appeared in 1904.

[7] See Naples and die!

scrofulous glands, too, arrived from the surgical clinic; the caseous masses were immediately placed in four camerae anteriores. But alas! After three and a half weeks, the eyes looked quite fresh and unchanged—and the two rabbits were given up and put back into the yard. But when I nevertheless examined them once again some days later, all four eyes showed the most beautiful tuberculosis of the iris. Now the experiments were continued on a large scale and Cohnheim was intensely interested in the result. Every day when he arrived, his first question was: "Nothing new from Paris?" And I had to demonstrate to him exactly our day by day observations of the development of the tubercles. A little rabbit with specially beautiful nodules was called "the pride of the family." At the same time, Welch had started with Cohnheim the famous experiments on pulmonary edema which were later reported in *Virchow's Archiv*.[8]

Since I also had work to do outside the pathological institute, in clinics, collections, etc., I had to get up very early. When I had eaten my breakfast on the way to the institute in one of the many "gardens," which for no scientific reason are divided into beer, coffee and milk gardens, and where lower middle class families often had breakfast, I was at work at seven o'clock. After dinner,[9] Welch and I returned to the institute. When our experiments were in full swing we worked with redoubled zeal, often till seven or eight o'clock in the evening. "It's never been as crazy as this before," said Höhndorf, the concierge, one day when, in the course of three weeks, he had had to buy 85 new rabbits for us. It was not as easy in the case of guinea pigs: I had to go myself to the zoo to beg six miserable guinea pigs in exchange for good words and considerable money, and of these only four were usable in the end.

While Welch was able to finish his work on pulmonary edema before the end of the semester, my experiments could not be brought to a satisfactory conclusion in Breslau, because of the long incubation period and the many problems involved. They were continued in Copenhagen after my return home and published in the *Nord. med. Arkiv* in 1878.[10] A preliminary notice is, however, to be found in the transactions of the Schlesische Gesellschaft für vaterländische Kultur, at the July 13th session

[8] W. H. Welch, Zur Pathologie des Lungenödems, *Archiv für pathologische Anatomie und Physiologie und für klinische Medicin*, 1878, vol. 72, pp. 375-412.

[9] "Mittagessen"; cf. footnote 2.

[10] This probably refers to Salomonsen's article, Om Indpodning af Tuberkulose, særligt i Kaninens iris, *Nord. med. Ark.*, 1879, vol. 11, nos. 12 and 19 (*Index-Catalogue of the Library of the Surgeon-General's Office*, vol. 14, Washington, 1893, p. 826 f).

of which Cohnheim demonstrated the "pride of the family" and a few other animals.[11]

Welch and I often took part—I might almost say regularly—in the sessions of the various medical societies. The meetings often took the most agreeable social forms, such as excursions into the beautiful surroundings. Thus we had a standing invitation to the medical sessions of the "Vaterländische." This famous society, whose full name is "Schlesische Gesellschaft für vaterländische Kultur," had grown up entirely out of the midst of the Breslau bourgeoisie, in difficult times (1803), and without princely privileges, and had in its youth, through its spirit of restless investigation, through its courage in attacking large tasks in spite of small resources, through purposeful intervention in public life, gradually won for itself a good position among the scientific associations of Germany. Here we heard interesting communications from Cohnheim, Freund, Weigert, Lichtheim, Hermann Cohn, Grützner and many others. The society's most enjoyable meeting was an excursion of the botanical section to which Ferdinand Cohn invited us. In a first-class compartment meant for nine people, I travelled to Friedland with thirteen other members of all ranks. Among the participants was Heinrich Haeser to whom I was able the more easily to introduce myself, as my father had given me a book which he had written years before about epidemics in Copenhagen in the 18th century, and which I was to deliver to Haeser. Haeser was a very voluble old gentlemen, which was of benefit to the society on this occasion, for the journey to Friedland took four full hours because of an accident to the locomotive. When I saw Haeser the next time in Breslau, it was in quite peculiar circumstances: in spite of his 65 years he was sitting as a bass among the 553 choir members in the second Silesian music festival in June!

At last we arrived in Friedland, were all seventy met in beautiful equipages, and soon we were in Görbersdorf, where we were received by the "hero of the district," the distinguished, serious, taciturn Dr. Brehmer. After lunch, the session. The president was, of course, old Göppert, who had already taught at the university for 50 years; the secretary was Ferdinand Cohn. Both gave short lectures, the latter demonstrating the Koch bacteria photographs. After the meeting, we went on foot to the "Tower of Joy" and back. Dinner with numerous speeches, coffee at Dr. Breh-

[11] The *Verhandlungen der Schlesischen Gesellschaft für vaterländische Kultur* were not available to me, nor have I found any reference to a publication besides Cohnheim's demonstration.

mer's who had promised to show the physicians the sanatorium. But the time was too short and Dr. Brehmer too retiring. A thorough inspection was impossible, but one got a good idea of the grandeur of the whole arrangement. None of us suspected—not Dr. Brehmer either, I'm sure—that we were standing here at the cradle of the tremendous sanatorium movement of a later period. Two eminent Breslau men whom, to my great joy, I met personally on this excursion, were the anatomist, Auerbach, and the chemist and wine merchant, Moritz Traube, whose very famous "cell" played so great a part in the development of physical chemistry.[12]

Soon, however, Welch and I exchanged our dignity as botanists for a role as ophthalmologists! We were invited to the ophthalmologists' excursion. On Sunday afternoon we went to Dührnfurth. In the beautiful park of the castle, opposite an old mausoleum, we camped on the grass under the presidency of Förster. This time the secretary was not "plant-Cohn" but "eye-Cohn" (Hermann Cohn). A few statistical and casuistic communications were made. Then, past the old Jewish cemetery, we went to the one decent inn in the place; here during the meal, a "palpebra or eye-song"[13] was first sung; later, a song in which Cohnheim's disciples were specially welcomed and the society, because of the beautiful harmony among its members, was solemnly christened "synechia circularis."

Welch and I took part in the big clinical excursion too. On this occasion I made my first acquaintance at lunch[14] with the peculiar German "Burschenkomment."[15] Later we dined in the finest hotel in Salzbrunn with Biermer "in the chair." There were songs and Bierzeitung[16] and a very long speech in which a physician of the local spa enlightened us on the splendid effects of the Salzbrunn waters, particularly for "phthisis pituitosa!"

Finally, I must mention the Physiological Society. Here the distinguished and progressive younger generation in the medical world gave their lectures. At the beer evenings which usually followed the sessions,

[12] Moritz Traube, Experimente zur Theorie der Zellenbildung und Endosmose, *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*, 1867, pp. 87-165.

[13] "Augenlied": the German word offers a pun on Augenlid = eyelid.

[14] "Beim Frühstück": here, not the light, early breakfast, but a meal later in the morning.

[15] The rules and customs governing convivial meetings of German student fraternities.

[16] Humorous newspaper put together to be read at a convivial meeting of German students.

the assistants of the clinical and theoretical institutes were regular habitués: Gscheidlen, Born, Grützner, Baer, Weigert, Lassar, Lichtheim, etc. Through them I became better acquainted with the various institutes. Gscheidlen at that time was continuing the Semper investigations into the influence of space on the size of animals and was working with astonishing thoroughness on his unfortunately never completed physiological technique. From Born I learnt the method of reconstruction which he had just worked out and became acquainted with a new growth within the personnel of the institute: for the first time I saw one of the "microtome-ladies" so much favored later on! Baer I met rather oftener: he was working on amblyopia centralis and I translated for him orally the book by my countryman, Krenchel, on this disease.[17]

While in these various scientific societies I came in touch with many medical men and scientists, in the home of Cohnheim and his charming wife I had an opportunity to associate with scholars from other faculties. To the permanent Cohnheim circle belonged Dilthey, the philosophy professor, and the two jurists, Gierke and v. Bar, who were later so famous. The latter Cohnheim introduced to me with the words: "Professor v. Bar. He is only a jurist but deserves to be a medical man." Luigi Brentano, the national economist, who looked just as Italian as his name sounds, was also among the most frequent guests of the Cohnheim family. To him Cohnheim introduced me as a young Danish medical man "who has read your strike bible"—that was the flattering name given to Brentano's famous book, "Workers' Guilds of Today," [18] which, to Cohnheim's great joy, I had studied in its entirety when, around 1870, the labor question became urgent in my fatherland, too. The only medical man whom I met personally in the Cohnheim house was Haidenhain whose clear, concise course on animal heat, accompanied by beautiful experiments, I followed weekly. Once, Welch and I were invited to a little excursion to Pirscham with the above named professorial families, Haidenhain, and his good sons. There we drank much good wine. We travelled there and back in three very primitive Ohle [19] boats, which, in spite of their completely flat bottoms, were continually in danger of running aground in the shallow river.

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[17] Valdemar Krenchel, *Amblyopia centralis*, Kopenhagen, 1876.
[18] Lujo Brentano, *Die Arbeitergilden der Gegenwart*, 2 vols, Leipzig, 1871-72.
[19] Tributary of the Oder.

I was very much impressed by the institutes of the Breslau university, for in Copenhagen our medical institutes were in a very bad way. When Panum was called to Copenhagen in 1863, he succeeded in establishing a physiological laboratory and to this institute, very spartan in its equipment, there streamed everyone—physiologists, pathologists, botanists, etc.—who wanted to work in experimental biology. No real pathological institute existed. There was, of course, a collection of pathological-anatomical preparations, but dissections were performed in a stinking little old house in the courtyard of the Friedrich Hospital. For his microscopic courses the pathologist had to seek refuge in Panum's physiological institute. There were no real work-rooms; a corridor or anteroom off the auditorium in the old surgical academy functioned as such, and the tiny room was at the same time cloakroom and washroom for all the many professors who lectured at different times in the auditorium. Under such conditions, Cohnheim's not particularly magnificent laboratory was bound to seem like paradise to me and I sent home detailed, enthusiastic descriptions of the various installations, even of the quite un-modern normal anatomy department which had found shelter in an old cloister. The vast old refectory, with its two long rows of pillars and beautiful pointed arches, was used as a dissection room and even over the courtyard of the institute with its half bare lawns—rudiments of the cloister garden—there still hovered the atmosphere of an Italian cloister, even though the former scent of flowers had long since been driven out by other exhalations.

In this old cloister building I, too, had to work for a short while. For, one day, the famous anatomist and pathologist, Adolf Hannover, of Copenhagen, sent me some drawings of hemicephalic and other crania which he had made in Breslau in 1839, in his 25th year, and asked me to find the originals in the anatomy department and to correct the drawings if necessary. He wrote that his time in Breslau, where he had associated with Otto, Henschel, Purkinje, Göppert and Barkow, had been just as instructive as it was pleasant. "The Breslau people were genial folk, a complete contrast to the dry people of Berlin." Otto's collection of malformations was the best German one of the time; Meckel's in Halle, an absolute lumber-room. From young Professor Hasse (who, in parenthesis, spoke Danish as fluently as I) I received permission to work in the anatomical museum. I easily found the old crania, revised the drawings as well as I was able, was obliged, however, in the end to get help from a professional draughtsman. In this way I sometimes had an oppor-

tunity to wander alone in the large museum galleries among a whole string of skeletons of beheaded criminals which Barkow had collected with particular interest, among very mediocre corrosion preparations—the remains of his famous "angiological collection"—and among the numerous beautiful skeletons of the zoological museum.

This tremendous superiority of the university of Breslau over the university of Copenhagen in the realm of the organization of medical institutes, I felt as a humiliation; all the more did I derive national satisfaction from the realization that in one important point the Breslau men were behind us. In April I wrote to my father: "I have found out that Fischer is not an antisepticist"—and this was quite inconceivable to me. It is true, I knew that the Danish surgeons were the first to adopt the antiseptic method and that Lister on this account cherished a certain feeling of gratitude to my fatherland. In a letter of 1902 in which he discussed a possible trip to Denmark, he wrote me: "I well remember that my old friend Professor Saxtorph was the first of continental surgeons to adopt the antiseptic system!" As early as 1867, that is ten years before my Breslau time, I had been trained in antisepsis as a student in Professor Holmer's surgical department and had with great care prepared the now forgotten carbolic oil chalk paste. With Bardeleben in Berlin I had seen antisepsis practised; even in the Cohnheim institute, they "Listered," as it was then called, in particularly extensive vivisectional operations. I was therefore surprised to find a non-antisepticist here in a clinical chair and, moreover, a surgeon of deservedly high esteem as a practitioner and teacher. In this sphere, however, still greater surprises were reserved for me later in Paris. The matter was discussed in the institute when Lichtheim brought back with him from a visit in Halle a few copies of Volkmann's "Preliminary Notice"[20] about the wonderful results of his operations. I immediately sent a copy to Copenhagen with the comforting remark: "I cannot believe otherwise than that antisepsis will 'endure,' as they say."

In the early morning hours, moreover, I followed Fischer's clinical lectures and examinations, often with great interest, and tried to acquaint myself, at least en passant, with great junior and senior clinicians in Breslau. I heard some lectures by Freund and Förster, went several times to Biermer, who was a typical Bavarian and an excellent teacher,

[20] Richard von Volkmann und Paul Kraske, *Vorläufiger Bericht über die innerhalb der letzten drei Jahre in der chirurgischen Klinik zu Halle stationär oder poliklinisch mit Hülfe der antiseptischen Methode behandelten schweren Operationen und schweren Verletzungen*, Leipzig, 1877.

visited studiously the demonstrations of the young neurologist, Berger, who had magnificent patient material at his disposal. He was, for instance, once able to show us three " scléroses en plaques " at the same time, representing three different stages in the disease. Just once I heard a clinical lecture by Spiegelberg. He was a small man, dressed in black, with black hair and moustache, white waistcoat, and patent leather boots, who, with a contented smile, made continual jokes. Suddenly he began to scoff at German surgeons for their dishonesty—the so-called " German honesty," he declared, was simply humbug—and, perhaps in order to do justice to all, he found an opportunity to adduce the famous quotation from Shakespeare's Hamlet: " Something is rotten in the state of Denmark." When, by chance, Königsberg was mentioned, he said suddenly: " Is anyone here from Königsberg? " A young student rose and Spiegelberg now started a conversion with him about the roads around Königsberg and the paving in the town, where, years before, he had been professor. In short, it was a very amusing and stimulating performance.

When my departure was approaching I discovered that I did not yet really know Breslau. Its many sights had escaped me: institute and studies had engulfed me completely. I was familiar with the surroundings and the Riesengebirge, more than with the town—we went there occasionally, whenever we had time, Welch and I, with German colleagues with whom we were friendly. Once we went via Kamenz to Landeck, where by bright moonlight we passed the " Ring " in an open carriage, just as the clock on the beautiful old townhall was striking two; and four strong, we had to run around from hotel to hotel in Bad Landeck in the night, and only after a long search did we find very modest quarters, in order, on the next day, to undertake a wonderful forced march through Seidenberg and Wölfelsfall. Another time we went with Lichtheim as leader to the mining and socialist center of Waldenburg, and then on over Langeberg and the Heck for a second visit to Görbersdorf, where, this time, we admired the model stables in the sanatorium. In large, elegant, clean halls the well-nourished cows stood side by side in rows.

Welch and I undertook a longish trip on foot from Hirschberg: first to Warmbrunn, then, on a country road to the mountain and castle of Kynast, rich in legends. Later on this trip we came to Josephinenhütte and in the glass-works we studied the history of the whole development of Rhine wine glasses. I have forgotten the technique, but the pale yellow,

thin, cachectic workmen and their miserable children I still see with uncanny clarity before my eyes. With an old guide we walked further in Rübezahl's [21] old country, but snow and ice forced us to spend the night in the first shelter on the Riesenkamm—the new Silesian hut—and later to start back to Breslau via Schmiedeberg.

In the period just before my departure the tuberculous nodules abounded in my rabbits' eyes, but I had to forsake them now. I entrusted them to the care and surveillance of my dear friend, Weigert, who did indeed observe them with the greatest care and sent me bulletins to Paris and Copenhagen. At last, I got my things in order in Breslau, attended a German doctoral disputation for the first and perhaps last time, and, after a hearty farewell carouse with Weigert, Lassar, Rosenbach, Welch, Gscheidlen, Grützner, Fick and some others, I went on August 1 to Prague.

"I treasure with the keenest delight the memory of the wonderful days in Breslau," Welch wrote me a few months ago. The full summer semester of 1877 is still inscribed in my heart, too, and I am firmly convinced that if Ehrlich looks back today on his student days, he will think of our common teacher with the same love and admiration as Welch and I. The enthusiasm which he inspired in his pupils and the brisk life of research which developed under his wing in the Breslau pathological department joined his people together, not only on a scientific, but also on a human, personal level.

Weigert and Lassar with whom I associated daily in the laboratory, early and late, remained my close friends until their deaths. But my more casual meetings with Robert Koch, Neisser, Lichtheim and others grew later into friendly relations. And so, too, with Ehrlich. As an old foreigner I did not exactly have much to do with the young Silesian student and in the subsequent years we did not meet at all. When I saw him again, he was assistant to Frerichs. I visited him in his modest apartment—in the Charité, I believe—where the table was covered with a huge pile of microscopic preparations, thrown together pell-mell, in the small hospital laboratory, where he demonstrated to me the most glorious new vital stainings, and on his evening rounds, where it was clear that he

[21] Rübezahl: the mountain spirit of the Riesengebirge, the subject of numerous legends.

did not lack clinical acumen either. Later, I was his guest in Steglitz, where his genius had transformed a small peasant's farm into a world center for immunization theories. Then I saw him in Frankfurt at the opening ceremonies of the institute for experimental therapy. With nervous fervor he expounded his theories from the speaker's desk, behind him the beautifully colored schematic charts with receptors and side-chains. Concerning these last, a not completely informed colleague behind me whispered to his neighbor: "Oh! Is that what they look like?" I met him again in Paris in 1900, where he defended his doctrine of immunization against the French school at the international medical congress. When, one day, I sought him out there, I saw immediately that I had not made a mistake in the hotel, for in the lounge several notes, torn off a pad and covered with colored drawings of amboceptors and complements, lay on the floor!! Several times I had the pleasure of seeing him in Copenhagen. In 1902 he came as leading investigator in the field of serology to bless with his presence the Danish State Serum Institute. Once later, on the way to Stockholm where he was to share the Nobel prize with Metchnikoff—a whole prize would certainly not have been too much for each of the two gentlemen!—Once again I saw him in Frankfurt, when the Georg Speyer house stood completed beside his former institute—a witness to his last and perhaps greatest feat—the creation and foundation of chemotherapy. Unfortunately, it was not granted to me to see Paul Ehrlich again last year in London, where the huge international medical congress paid homage to him as the "facile princeps" of living biologists.

In one of his first chemotherapeutic publications, Ehrlich writes that he would like some time to finish a work de causis et sedibus pharmacorum. We accept the omen! Giovanni Baptista Morgagni was 79 years old when he published his work de causis et sedibus morborum. So we shall expect great things from Paul Ehrlich's pen in the year 1933, and we shout to him today from the bottom of our hearts: Ad multos annos!

BIOGRAPHICAL INDEX [22]

AUERBACH, LEOPOLD (1828-1897), anatomist and neuropathologist, taught at Breslau from 1863.

BAR, KARL LUDWIG von (1836-1913), jurist, professor at Breslau University from 1868 till 1879 when he went to Göttingen.

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[22] This index is merely intended for quick orientation as to the persons mentioned, especially with regard to the year 1877.

BARDELEBEN, ADOLF VON (1819-1895), professor of surgery at Berlin from 1868, early adherent of the antiseptic method.

BARKOW, HANS KARL LEOPOLD (1798-1873), anatomist at Breslau from 1826 till his death.

BARTELS, KARL HEINRICH CHRISTIAN (1822-1878), professor of medicine at Kiel University from 1851 till his death.

BERGER, OSKAR (1844-1885), neurologist, taught at Breslau University from 1873.

BIERMER, ANTON (1827-1892), professor of medicine in Breslau from 1874.

BORN, GUSTAV JAKOB (1851-1900), anatomist, at Breslau University from 1873.

BREFELD, OSKAR (1839-1925), studied pharmacy, turning later to chemistry and botany, was one of the early bacteriologists.

BREHMER, HERMANN (1826-1899), physician, founded sanatorium for tuberculosis patients at Görbersdorf. This was the beginning of mountain sanatoria.

BRENTANO, LUJO (1844-1931), German national economist, professor in Breslau 1872-1882.

COHN, FERDINAND (1828-1898), professor of botany in Breslau.

COHN, HERMANN (1838-1906), physicist and chemist, then studied medicine, ophthalmologist at Breslau University from 1868.

COHNHEIM, JULIUS (1839-1884), professor of pathological anatomy in Breslau 1872-1878; then in Leipzig.

EHRLICH, PAUL (1854-1915). His early work dealt with histologic staining; later he turned to immunology and chemotherapy.

DILTHEY, WILHELM (1833-1911), German philosopher and historian, was professor in Breslau from 1871 till 1882 when he went to Berlin.

EIDAM, EDUARD (1845-1901) became Ferdinand Cohn's assistant in 1873.

FICK, ADOLF GASTON EUGEN (1852-), ophthalmologist, in 1877 was assistant at the anatomical institute in Breslau.

FISCHER, HERMANN (1831-1919), professor of surgery and director of the surgical clinic in Breslau 1868-1890.

FÖRSTER, RICHARD (1825-1912), ophthalmologist, at Breslau university from 1857.

FRERICHS, FRIEDRICH THEODOR (1819-1885), professor of medicine in Breslau 1851-1859, then in Berlin.

FREUND, WILHELM ALEXANDER (1833-1918), obstetrician and gynecologist at Breslau University from 1857 till 1879 when he went to Strasbourg.

GAFFKY, GEORG (1850-1918), army physician, appointed with Loeffler to assist R. Koch in Ministry of Health, Berlin, 1880.

GIERKE, OTTO (1841-1921), jurist, professor in Breslau 1872-1884.

GÖPPERT, HEINRICH ROBERT (1800-1884), physician and botanist, at Breslau University from 1827.

GRÜTZNER, PAUL von (1847-1919), studied medicine at Breslau, Berlin, Vienna, for some time was assistant in Breslau physiological institute, professor at Bern 1881, at Tübingen 1884.

GSCHEIDLEN, RICHARD (1842-1889), became assistant at the physiological institute at Breslau in 1869.

HAESER, HEINRICH (1811-1884), famous medical historian, from 1862 professor in Breslau.

HAIDENHAIN, *see* HEIDENHAIN.

HANNOVER, ADOLPH (1814-1894), physician and anatomist in Copenhagen.

HASSE, KARL (1841-1922), became professor of anatomy in Breslau in 1873.

HEIDENHAIN, RUDOLF (1834-1897), from 1859 professor of physiology in Breslau; his sons were Lothar H. and Martin H.

HENSCHEL, A. W. E. TH. (1790-1856), professor at the university of Breslau, outstanding for his work in medical history.

HOLMER, VALDEMAR (1833-1884), surgeon in Copenhagen.

KLEBS, EDWIN (1834-1913), pathologist, early protagonist of bacteriology, professor in Prague 1873-1882.

KOCH, ROBERT (1843-1910), district physician in Wollstein 1872-1880, member of the Kaiserliches Gesundheitsamt in Berlin 1880-1885, professor of hygiene at the University of Berlin 1885, director of the Institut für Infektionskrankheiten 1891.

KRENCHEL, VALDEMAR (1844-1885), ophthalmologist in Copenhagen.

LASSAR, OSCAR (1849-1907), assistant to Cohnheim at the pathological institute in Breslau 1875-1878, became dermatologist in Berlin.

LICHTHEIM, LUDWIG (1845-1928), in Breslau 1873-1877 at the policlinic, called to Jena 1877.

LISTER, JOSEPH (1827-1912), his first publication on antiseptic surgery appeared in 1867.

LOEFFLER, FRIEDRICH (1852-1915), associated with R. Koch in bacteriological work in Ministry of Health, Berlin, 1880; later professor of hygiene in Greifswald.

LUDWIG, CARL (1816-1895), professor of physiology in Leipzig 1865-1895.

MADSEN, THORVALD (1870-), Danish bacteriologist.

MECKEL, JOHANN FRIEDRICH (1781-1833), anatomist in Halle, enlarged the anatomical collection of his father and grandfather.

MEISSNER, GEORG (1829-1905), professor of physiology in Göttingen from 1860.

METCHNIKOFF, ELIE (1845-1916), became director of the Institut Pasteur, Paris, 1904; Nobel prize with Paul Ehrlich 1908.

MORGAGNI, GIOVANNI BATTISTA (1682-1771), *De sedibus et causis morborum* appeared in 1761.

NEISSER, ALBERT (1855-1916), dermatologist, discovered the gonococcus.

NOTHNAGEL, HERMANN (1841-1905), professor of clinical medicine in Jena 1874-1882.

OTTO, ADOLPH WILHELM (1786-1845), from 1813 professor of anatomy in Breslau.

PANUM, PETER LUDWIG (1820-1885), famous professor of physiology at Copenhagen.

PURKINJE, JOHANNES EVANGELISTA (1787-1869), physiologist and pathologist, called 1823 to Breslau university, where he founded physiological institute, called to Prague 1849.

ROSENBACH, OTTOMAR (1851-1907), physician at All Saints' Hospital, Breslau, from 1877; professor at Breslau university till 1896 when he retired to Berlin to do scientific work.

SALOMONSEN, CARL JULIUS (1847-1924), the author of this article, born in Copenhagen where he studied medicine. In Breslau for further study under Julius Cohnheim 1877; university career in pathology and bacteriology in Copenhagen; 1893, appointed professor of general pathology and director of serumtherapy laboratory, then of State Serum Institute.

SAXTORPH, MATHIAS (1822-1900), Danish surgeon, professor in Copenhagen and head surgeon at King Frederick Hospital.

SPIEGELBERG, OTTO (1830-1881), obstetrician and gynecologist, called to Breslau professorship 1875.

TRAUBE, LUDWIG (1818-1876), pathologist and physiologist, founder of German experimental pathology.

TRAUBE, MORITZ (1826-1894), younger brother of Ludwig Traube, worked on semi-permeable membranes.

UNVERRICHT, HEINRICH (1853-1912), pupil of Biermer, M. D. 1877 in Breslau, later professor and director of the policlinic in Jena.

VIRCHOW, RUDOLF (1821-1902), the founder of cellular pathology.

VOLKMANN, RICHARD von (1830-1889), professor and director of the surgical clinic of university of Halle.

WALDEYER, WILHELM (1837-1921), pathologist in Breslau 1865, and anatomist, Strasbourg 1872, Berlin 1883.

WEIGERT, CARL (1845-1904), Cohnheim's assistant at the pathological institute, Breslau, 1874-1878; followed Cohnheim to Leipzig 1878.

THE HISTORICAL RELATIONSHIP BETWEEN THE CONCEPT OF TUMOR AND THE ENDING -OMA *

HARRY KEIL

> The medical historian has rarely drawn upon his fund of knowledge to enlighten us as to the history of words. His attentions are confined to the history of men and of ideas. He is inclined to forget that careful dissection of words used to express ideas may throw a flood of light upon obscure problems.
>
> (E. Andrews: *A History of Scientific English*. New York, Richard R. Smith, 1947, p. 7. By permission of the publisher.)

The ending -oma has been customarily applied to growths of neoplastic nature, whether benign or malignant, but it has also been used to designate swellings produced by inflammatory and metabolic diseases, such as paraffinoma, xanthoma, tuberculoma, syphiloma, and various granulomas of more acute as well as of chronic nature. Words ending in -oma therefore generally imply a tumor in the sense of swelling or elevation, irrespective of its nature. Contrary opinions have been expressed regarding the existence of a Greek suffix -*ωμα* (-oma) as recorded in many medical and other dictionaries. Leider and Rosenblum (1) have recently contended that -oma represents a corrupted form of the true Greek suffix -ma. In the opinion of these writers there has never been any historical basis for the use of -oma in the sense of a tumor. Contrariwise, Andrews (2) claimed that from early times -oma was a Greek suffix with the connotation of swelling; that, although it is now mainly restricted to neoplastic swellings in contradistinction to inflammatory ones, such words as haematoma bear witness to an earlier broader usage. In addition, Andrews cited Xenophon as having derived the word *sphairoma* (*buttock*) from *sphaira* (sphere), implying thereby that the ending -oma was intended to indicate an elevation or swelling. Whether the word *sphairoma* was coined by attaching -oma to a noun stem or -ma to a verb stem must be left to the decision of philologists. How far, also, the ancients and the moderns have coined such terms by the simple principle of analogy is often difficult to determine with certainty. In any

* From the Montefiore Hospital for Chronic Diseases, Department of Dermatology.

event, these opposing camps have not furnished definite historical data to show the probable manner in which this ending came to be applied widely in medical nomenclature.

Although medical terms ending in -oma have been known from antiquity, only in the past one-hundred and fifty years has the medical nomenclature become enriched with such words. The practice of coining names in this way is therefore of relatively recent vintage. Whether or not this suffix existed in ancient Greece, it can be shown on historical grounds that it could not have been used as designating a new growth, for no such concept was held until about the middle of the nineteenth century. The purpose of this article is to supply historical data indicating the manner in which this ending evolved and how it came to be applied in its present broad usage. Moreover, it will be shown that the ending -oma represents a modern clinico-pathologic modification of the Galenic conception of the "tumors against nature" (3). Throughout the ages physicians have sought to use some simple word or expression for the class of pathologic swellings, and so we shall have to discuss the *oedema* of the Hippocratic writers, the mass or oncos (ὄγκος) of Galen, the *apostema* of the Latin translators of Arabic authors, the "tumors against nature" of the Renaissance, the *abscessus* of Severinus (1632), the *phyma* of Sauvages (1763), the simple tumors of other writers, and the -oma of the moderns of the last century and a half. We shall begin our discussion with some remarks on the evolution of the concept of cancer and carcinoma, since this has an important bearing on the history of the ending -oma.

I.

The Hippocratic writers (fifth to fourth century B. C.) used the words *carcinos* (καρκίνος) and *carcinoma* (καρκίνωμα) in a practically synonymous sense (4a), and both meant crab. These words were nowhere precisely defined, and they were applied indiscriminately to intractable ulcerations as well as to lesions that seem to correspond to our modern malignant neoplasms. To my knowledge there is no evidence that the ending -oma in the word carcinoma had the specialized, restricted connotation of a swelling or tumor, nor is there evidence that this term was applied only to malignant tumors of neoplastic nature. Knuckle-shaped or knobby lumps outside the anus were known to the Hippocratic writer of the treatise "Hemorrhoids" as *condylomata* (κονδυλώματα) (4b), although it is not entirely clear that a sharp differentiation was always made from ordinary hemorrhoids. The word *ecchymoma* (ἐκχύμωμα)

(4c) was applied by the ancients as a synonym for *ecchymosis* (ἐκχύμωσις) or *peliosis* (πελίωσις) (4h) in the sense of an extravasation of blood under the skin, but it seems unlikely that the former term had the restricted connotation of a swelling induced by such extravasations. To describe lividity of the genital parts and on the chest, the Hippocratic writers used the expression *pelioma* (πελίωμα) (4d). Daremberg (4d) stated that the word *peliomata* was applied by the ancient Greeks to superficial livid spots announcing the imminence of death. The context of the foregoing citations affords no substantial basis for believing that *peliomata* implied swellings, and on clinical grounds, such appearances are not accompanied by elevation. Again, *helkos* (ἕλκος), a word often found in the Hippocratic treatises in the sense of ulceration, superficial erosion or sore, was apparently synonymous with the term *helkoma* (ἕλκωμα) (4i).

In a few passages in the Hippocratic writings (4e) the term *oedema* (οἴδημα) seemed to have the connotation of swellng or tumor, and, according to Galen (5a), this expression was employed by Hippocrates as a general term for the "tumors against nature," that is to say, the pathologic swellings. In Epid. VII, 106, for example, the Hippocratic writer vaguely recorded an eruption in an infant of two months. The eruption appeared on various parts of the body and was composed of red elevated tumors or swellings (οἰδήματα). The description is too succinct to allow a guess as to the nature of the dermatosis. It is however clear that the term *oedema* did not then have its modern, more restricted meaning, but by the time of Galen (second century A. D.), this word had more or less acquired its present significance, at least in a clinical sense. The Hippocratic writers also used the term *phyma* (φῦμα) (4f), and in the course of centuries this word took on the vague meaning of tumor, growth, or tubercle. There is overwhelming evidence, however, that the Hippocratic writers, who first used the word *phyma* in a truly medical sense, meant a variety of swelling that was attended with the formation of pus, such as abscess, phlegmon, suppurating glands, or even a simple pustule. This connotation of a suppurating lesion was maintained in one form or another for centuries, although the meaning was distorted toward the end of the eighteenth century. Likewise, when Herodotus (6) recorded how Democedes had accomplished a rapid cure of a *phyma* in the breast of Darius's wife, common sense would urge that the lesion was probably a mammary abscess and not a malignant neoplasm.

The Hippocratic concept of *carcinos* and *carcinoma* underwent a radical

change in the hands of Celsus (first century A. D.). Translating the former into the Latin *cancer* (meaning crab) and transliterating the latter into the Latin as *carcinoma* (a word foreign to the Romans), Celsus gave these terms different interpretations (7a). Cancer meant generally a deeply penetrating type of ulceration, whereas under *carcinoma* Celsus furnished a remarkable discussion of what would correspond to premalignant and malignant neoplastic lesions. However, this distinction between *cancer* and *carcinoma* was not followed by subsequent authors. In additon, Celsus differentiated a number of benign growths, all amenable to surgical treatment, such as the *atheroma* (8) (*ἀθάρη*, porridge), and *steatoma*, evidently named from some form of the Greek *στέαρ* (stear), meaning fat. That the ending -oma had not necessarily the special significance of swelling or tumor is shown by Celsus' (7c) referring to a type of phagedenic ulcer, which the Greeks had already named *therioma* (*θηρίωμα*). This writer also used the Latin term *tumor* in the simple sense of swelling, without any other special connotation. It was, for example, one of the four signs in the Celsian concept of inflammation: "Now the signs of inflammation are four: redness and swelling (*tumor*) with heat and pain" (7d). Likewise, the word *tuberculum* meant swelling or protuberance, and Celsus (7e) used it in a sense more or less equivalent to that of *tumor*. Both expressions, *tumor* and *tuberculum*, were destined later to receive other interpretations.

Celsus (7f) also transliterated several Greek terms ending in -cele (*κήλη*), this ending having been generally employed in the sense of a hernial swelling. Examples of this usage were words such as *enterocele*, *epiplocele*, and *bubonocele*. However, this ending was also used with the more general meaning of tumor or swelling, as in *bronchocele* (a heterogeneous concept), *cirsocele* (our varicocele), *sarcocele* and *hydrocele*. These expressions seem not to have been used by the Hippocratic writers. It is likely, therefore, that the words were coined in the interval between the fourth century B. C. and the first century A. D. Many such terms survived for centuries; however, some were later modified in meaning, others were replaced by more suitable names, and still others were dropped altogether. It may be noted that the keloid of Alibert (9) (more correctly denominated as cheloid) and Addison's keloid (10) (now known as morphea or circumscribed scleroderma) have no direct relation to the Greek *κήλη* (tumor, hernia).

Galen reverted to the Hippocratic use of *carcinos* and *carcinoma* as synonymous terms (5b). To this observer we are indebted for the im-

portant and influential concept of the "tumors against nature." Under the heading *oncos* Galen (3) included pathologic lesions showing deviations in length, width, or depth. Since knowledge was limited chiefly to changes visible to the naked eye, the "tumors against nature" comprised a large number of cutaneous diseases, and in his treatise Galen (3a) envisaged a concept of inflammation that was more surgical in nature than the one of Celsus. In short, this treatise was concerned not only with affections of the skin, but also with minor surgery and conditions that we now consider as belonging to the realm of major surgery. The term *sarcoma* (5c) was applied to flesh-like lesions in the nose, including polyps. The nature of this anomaly, as defined by Galen, is far from certain, and the meaning adopted by subsequent observers is also vague. Thus, Fernel (11a) (1554) included in this category such diseases as polyp of the nose, epulis of the gums, and probably condyloma acuminatum (*σύκωσις* or *ficus*) around the anus. Severinus (13a) (1632) spoke of *myxosarcoma* when he seemed to mean elephantiasis of the scrotum. Lorry (14a) (1777) applied the term to fleshy swellings in the skin. Abernethy (15) (1804) used it for malignant neoplasms, but undoubtedly his concept included cancers of epithelial and glandular origin. The meaning of the term sarcoma remained obscure until the time of Virchow (101a).

Galen therefore used the expression *oncos* in the sense of swelling or tumor, pathologic in nature, arising from various humors or combinations of humors, and more or less equivalent to the *oedema* of the Hippocratic writers. It is interesting that he also occasionally employed the word *oncoma* (*ὄγκωμα*) (5d) but this word was probably not the basis for the subsequent wide adoption of the ending -oma in the sense of neoplastic or other types of growth. In the first place this term, which was synonymous with *oncos*, was but rarely used by Galen. Then again, in de Gorris' dictionary (57) of medical terms (1578), which is said to have had an enormous influence on the nomenclature of succeeding authors, the word *oncos* was only mentioned cursorily in a discussion of the meaning of the word *oedema*, and no reference was made to the term *oncoma*. Even in Thevenin's etymologic dictionary (1691), in which *oncos* was directly defined, *oncoma* is nowhere to be found.

Galen's writings also contained other terms that were occasionally used in the sense of elevation, swelling, or tumor, but, among these, only the word *blastema* (*βλάστημα*) (5e) partially survived in medical nomenclature. In his treatise on the tumors against nature, *blastema* referred to

an excrescence (5e) or elevation as observed in a type of wart or in a polyp. The Hippocratic writer (4g) of "Humors" had referred vaguely to *blastema*, a word that Littré translated as excrescence. The term *blastemata*, found in Aretaeus (16), was rendered as "eruption" by Adams, but the noncommittal word "pimples" would have been more accurate since Aretaeus was concerned with a comparison between the cutaneous effects of thapsia (a strong primary irritant) and the lesions of *ionthoi* (ἴονθοι) (our acne vulgaris). In 1691, Thevenin (17) defined *blastemata* as tumors in the groin, which arose from superfluities in the abdominal organs, and he noted that others had used the term in the sense of exanthematic eruptions, as in smallpox and other "malignant pustules." The clearest definition consistent with the etymology of the word, was that of Hooper (1822) (18) who, noting that *blastema* meant bud or young shoot, interpreted the Hippocratic reference as indicating a cutaneous pimple like a bud. About the middle of the nineteenth century, the great advances in embryology made necessary the formulation of many words designating primitive structures, and for this purpose the root blast- was conveniently introduced in such words as blastoderm, blastopore, blastocele and the like. A short step led to the term blastoma in the sense of a neoplasm arising from embryonic rests, whereas some authors, such as the pathologist Adami (19), employed the expression for any tumor exhibiting the property of autonomous growth. In the latter sense, words of the type of lymphoblastoma, hemangioblastoma, and melanoblastoma were coined, but these formulations do not necessarily mean that the conceptions of these diseases had become more precise.

Galen's views on *cancer* (*carcinoma*) and on the "tumors against nature" prevailed in succeeding centuries. The Arabic authors enthusiastically embraced his general theories, often adding an additional scholastic touch; but they established their own nomenclature for *cancer* (*essarathan*) (20) and for the praeternatural swellings (*auram*, singular *waram*) (21). Steinschneider (22) showed that Galen's treatise, "On the Tumors against Nature," was known to the Arabic physicians. They translated *oncos* by the Arabic word *waram*, as is demonstrated in the pertinent chapters in Haly Abbas (tenth century) where the plural auram was specifically applied in the Galenic sense of the humoral swellings against nature (21). When, however, Rhazes, Avicenna, and other Arabic authors were first made available to Western Europe, the Latin translators, such as Constantinus Africanus (23) (d. 1087), Stephen of Antioch (24) (c. 1127), Gerardus of Cremona (25) (d. 1187), and even

Hieronymus Surianus (26) (late fifteenth century) introduced the Latinized Greek word *apostemata* as equivalent to *auram* and therefore equivalent to the *oncoi* of Galen or the humoral " tumors against nature." Since this new nomenclature remained in force for the next four centuries, and since the selection of this terminology involves questions of special interest, it will be useful to trace the evolution in the meaning of the term *apostema*.

II.

Although the etymology of *apostema* (meaning interval or distance) (27) hardly suggests the idea of suppuration, this term was often used by the Hippocratic writers in the sense of a deeply situated abscess, in which suppuration became evident when rupture occurred through the skin. In the language of Celsus (7i) the word *abscessus* (from L. *abscedere*, to go away, that is, the separation of contiguous parts) was etymologically equivalent to *apostema*. There is evidence (7j) that *abscessus* did not then have its present connotation but merely indicated congestion, which might either resolve or suppurate to form a genuine abscess. Galen seemed to have evolved a broader, if somewhat inconsistent, conception of *apostema*. In " De medendi methodo ad Glaucum " (5f) this author defined the term as a *diathesis* in which contiguous parts became separated, thus forming a space for the collection of fluid or air or both. Certain inflammations and many types of erysipelatous and phlegmonous swellings could become *apostemata*, and apparently encysted tumors, such as sebaceous cysts were also included within this category. Moreover, Galen described methods for resolution of the lesions without the advent of suppuration. It is clear that this interpretation did not necessarily imply our modern notion of abscess, at least in the sense of grossly visible pus. At the same time this interpretation was consistent with the broad etymology of *apostema* and its Latin equivalent *abscessus*. Yet, in Galen's treatise (28a), " On the Tumors against Nature," *apostema* seems mainly to have been applied to a deep purulent collection which ruptured through the skin, and Galen specifically noted that variants of this process were also called *empyema*, *diapyema*, and so forth. Oribasius (fourth century A. D.), in his large treatise on medicine (29a), adopted the broad conception of his predecessor and conserved the double meaning given to this term. By the seventh century Paulus Aegineta (30, 31) noted the confusion in the meaning of *apostema* and remarked that other physicians applied the expression *phyma* (in the sense of a suppurating lesion) to swellings of the nature of *apostemata* (30a).

In the interim the Latin writers in Western Europe, among them Pliny the Elder (32) (first century), Cassius Felix (33) and Caelius Aurelianus (34) (the latter two in the fifth century), used the term *apostema* in the more or less restricted sense of a collection of pus; and the same meaning was preserved in such early Salernitan authors as Petrocellus (35) (c. 1035) and Gariopontus (36) (c. 1050). It is noteworthy that these physicians preferred the Latinized Greek word *apostema* to the Latin equivalent *abscessus*, which Celsus had employed. This usage suggests that the Greek medical nomenclature had survived and had become incorporated, at least partially, into the Latin tongue during a period when the work of Celsus had fallen practically into oblivion. It has already been noted that the Latin translators, who had introduced the Arabic works into Western Europe at the end of the eleventh century and thereafter, rendered the Arabic *waram* as *apostema*. These books now referred to hot, soft and hard *apostemata*, large and small *apostemata*, natural and non-natural *apostemata*, and the like. Why did the translators choose the word *apostema* rather than its Latin etymologic equivalent, *abscessus*? The probable explanation is that Celsus was unknown to them, and, moreover, much of his terminology had not entered the medical nomenclature up to this time (37). More important, why was the expression *apostemata* used altogether in the broad sense of the "tumors against nature" when in Western Europe the word had practically always been restricted to suppurative lesions? The choice of terminology may have been influenced by knowledge of the broad definition given by Galen and by the lack of a suitable term in the Latin nomenclature to embrace the conception of the "tumors against nature."

The translator Constantinus Africanus exerted considerable influence on the nomenclature that ensued. Thus, the "Four Masters" (38) (end of the thirteenth century), in their commentary on the surgical texts of Roger (c. 1170) and of Roland (early thirteenth century), specifically cited Constantinus as defining *apostema* in the sense of a tumor or swelling of a part due to excess or superfluous humors. This definition was followed by the Italian, French, and other surgeons, such as Theodoricus (39), Bruno (40), Lanfranc (41), Henri de Mondeville (42a), Jehan Yperman (44), Guy de Chauliac (45a) and many others. It was common practice, also, for surgeons and physicians to speak of cancer or varieties of cancer as *apostema melancholicum* of a special type (Theodoricus (39b), Gilbertus Anglicus (46), John of Gaddesden (47), Bernard de Gordon (48) and others), and many men discussed *cancer* under two

separate headings: first, as an apostema and second, as an ulceration (Theodoricus (39a), Henri de Mondeville (42b), Guy de Chauliac (45b). In this field Henri de Mondeville stands out as an original and interesting author. For example, he noted that the illiterate and the vulgar restricted the term *apostema* to purulent lesions or to small pustules, whereas he preferred to follow Avicenna (actually in Latin translation) in including any enlargement against nature. That physicians were troubled by nomenclature was further shown when Henri de Mondeville declared: . . . " apostema, dubelech, tumor, eminence, enlargement, elevation, swelling against nature " are seven synonymous terms. When lesions suppurated, these varieties of *apostemata* received distinctive names according to the clinical and theoretical doctrines in vogue. Suppurative lesions were generally designated by a vocabulary rich in terms, such as *bubo*, *phygethlon*, *phyma*, *exitura*, *pustula*, *furunculus*, and the like. In the later Middle Ages, then, *apostema* came to mean any humoral tumor or swelling against nature.

During the Renaissance the term *apostema* in its generic sense was gradually replaced by, or made subservient to, the expression *tumores praeter naturam* (tumors against nature). The surgical treatise of Johannes de Vigo (1514) (49), which was published at the crossroad between the Middle Ages and the Renaissance, showed many features characteristic of his immediate predecessors, such as a defective acquaintance with Greek terms and their etymologies. The Galenic conception of the " tumors against nature " was covered in his first treatise in book 2, entitled " De apostematibus " (49a), which his English translator (1550) rendered: " Of the apostemes " (49b, 50). Tagaultius (1543) (51a) noted that many physicians and surgeons and even the vulgar comprehended under *apostemata* all the " tumors against nature," whereas others interpreted the term as *exitura* (abscess in the sense of suppuration) and as *pustula*. In view of these conflicting opinions, Tagaultius preferred to use the more general phrase *tumores praeter naturam*. Others, such as Leonus (52) (1562), Borgarucci (53) (1569), and Paré (54) (1575), continued to use the two expressions synonymously in the form of " tumores praeter naturam seu apostemata." Whatever the nomenclature, the praeternatural swellings were described during this period in essentially the same manner as in Galen and his successors, with some modifications here and there. The same doctrines were used in connection with hitherto unrecognized manifestations of disease. For example, the gumma (55) of *morbus gallicus* (*lues venerea*, *morbus venereus*, later known

officially as syphilis) was often described as a hard *apostema* caused by the deposit of modified phlegm (56a). In the celebrated "Pathologia" of J. Fernel (11b) (1554), *apostema* was defined on the basis of Galenic theories as a suppurative lesion or as a collection of foreign matter in various types of cysts, and this double meaning was also recognized by de Gorris (57) (1578), Joubert (58) (1642), and Castelli (59) (1746). Gradually by the end of the seventeenth century the term *apostema* was either abandoned, as in the surgical treatise of Wiseman (60) (1686), or, more commonly, it was reserved for a tumor tending to suppuration despite resolutives (Thevenin (17), 1691). In the famous *Traité des tumeurs et des ulcères* (1753), Jean Astruc (61) observed that *apostême* and *abscès* were synonymous terms indicating suppuration in a phlegmonous process, but that, although both words were in common use in France, the latter was the more usual one. Sauvages (62a) (1763) defined apostema as an encapsulated collection of pus or ichorous matter. This word is now rarely employed (64), although modern dictionaries define it as an abscess, an interpretation that affords no insight into the historical fluctuations of its meaning.

During the Renaissance the vast majority of surgeons and physicians preferred the phrase *tumores praeter naturam* to designate the various pathologic swellings. One of the earliest references to this expression is found in an incunabulum (1498) of Bruno's treatise on surgery (40), in which *apostema* is defined as a tumor or swelling against nature. By the middle of the sixteenth century this was the standard nomenclature used in the writings of Tagaultius (51) (1543), Fernel (11c) (1554), Crassus (65) (1562), Leonus (52) (1562), Fallopius (72) (1566), Borgarucci (53) (1569), Paré (54) (1575), L. Fuchs (66a) (1583), Fabricius ab Aquapendente (67a) (1592), and many others. Paré was probably the first to write this phrase in the vernacular, as he had little or no knowledge of Latin, the scientific tongue of this period. Although the generic heading was now "tumors against nature," cancer was still commonly described as an *apostema melancholicum*. At the end of the sixteenth century the terminology was simplified and the generic name became "tumor" without qualification (Gale (106), 1586; Wiseman (60), 1686; Thevenin (17), 1691; Deidier (68a), 1725; Astruc (61), 1759). It should be stressed that the term *tumor* designated merely a large class of pathologic swellings, which also included all manner of cutaneous diseases.

From time to time physicians have tried to improve the medical nomenclature and have only succeeded in creating more confusion. Among

these was Severinus (13b) (1632), who used the word *abscessus* in the broad sense which characterized the term *apostema* in the late Middle Ages. Under the term *abscessus* Severinus recorded the "tumors against nature," whereas suppurative lesions were generally designated by other terms. In all probability Severinus, casting about for a general expression, chose the Celsian equivalent instead of the Latinized Greek *apostema*. In the interval between the time of Celsus and about the end of the fifteenth century when the work of Celsus was printed in at least four editions (69), the term *abscessus* had practically disappeared from medical nomenclature. Celsus was read by Johannes de Vigo, Benivieni (70a, 71), Tagaultius (51b), Fernel (11), Fallopius (72), and Severinus (13c) among many others. When physicians and surgeons in the Renaissance became acquainted with Celsus and the original Greek works, nearly all recognized the equivalance of *abscessus* and *apostema* [Tagaultius (51a), Fernel (11), Fallopius (72), de Gorris (57), L. Fuchs (66b)]. Fracastor (73) (1546) was one of the few who had read Celsus but who apparently used the term *abscessus* in the restricted sense of a purulent process allied to phlegmon. In order to make himself clear, Fracastor had to speak of "pustular" abscess (pustulosus abscessus). Among the first to reject the term, *abscessus*, because of the confusion in meaning, was Tagaultius (51a) (1543). Joubert (58) (1642) also noted that this word, like *apostema*, embraced two separate concepts, the one meaning suppuration and the other, various types of cystic structures. This differentiation was further emphasized by Fabricius ab Aquapendente (67b) (1619) when he referred to *abscessus* with or without an enveloping membrane. By the end of the seventeenth century *abscessus*, like *apostema*, was defined in the restricted sense of purulent collections (Thevenin (17), 1691), and this definition became fixed in the common parlance of medicine (Deidier (68b), 1725; Astruc (61), 1759; David (74), 1778).

III.

In the eighteenth century, when attempts at classification reached extraordinary heights, Sauvages (1763) (63) tried to introduce the word *phyma* in the broad sense of "tumor against nature." This arbitrary usage had no genuine counterpart in the past. The Hippocratic writers (4f), as stated previously, used the word *phyma* in the sense of suppurative lesions in most passages where the meaning is plainly expressed. This is likewise true of Celsus (7g) except in a few places where the data recorded are too meager for accurate or reasonable interpretation. The

most direct reference to *phyma* is found in book V, 28, 9 (7g), where Celsus recorded the lesion in some detail. This description is sandwiched in between that of *furunculus* (boil) and *phygethlon*, both purulent processes. These data hardly permit any interpretation other than that *phyma* was a suppurative affection; in the words of Celsus: "Phyma vero nominatur tuberculum furunculo simile. . . ." ("A swelling similar to a furuncle is called a phyma. . . .") In this passage and several others *phymata* were described as *tubercula*, but the latter term expressed merely the conception of swelling in general. Galen defined a *phyma* as a spontaneously arising phlegmon that underwent rapid suppuration and affected chiefly the glandular structures (lymph nodes) in the groin and neck; and this was differentiated from *choirades* (χοιράδες) (28b), the *scrofula* or *struma* of the Latins. This differentiation represented a crude endeavor to separate the "hot abscess" of acute inflammatory nature from the chronic "cold abscess," a point of view in advance over that of the Hippocratic writers who had mentioned both affections, without attempting to seek the distinctive features of each. In describing a severe form of *dothien* (δοθιήν) or furuncle showing the property of penetrating the skin, Galen (28c) compared this lesion with *phyma* as a suppurative process. Galen's views on the acutely suppurative nature of *phyma* were accepted by subsequent Greek writers, such as Paulus Aegineta (30). During the Middle Ages in Europe, this expression seems to have disappeared from the Latin medical nomenclature. With the advent of the Renaissance and with increasing knowledge of the Greek language, the term *phyma* came back into usage. From this time until the early part of the nineteenth century, Galen's interpretation was accepted by such physicians as Fracastor (72), Fabricius ab Aquapendente (67), Wiseman (60b), Thevenin (17), Astruc (61), Lorry (14b) (1777), Willan, and Bateman (75). The last-named (1814), on the authority of Willan, named *phyma* a genus, in which were included terminthus, epinyctis, furuncle, and the "carbuncle of authors," all purulent lesions or diseases in which suppuration was a prominent feature. Bateman declined to describe them in his book on diseases of the skin, since he had nothing to add to the accounts given in surgical treatises. Nevertheless, during the Renaissance, a few powerful dissenting voices were instrumental in confusing the meaning of this word. Thus, Fernel (11d) (1554) classified the swellings against nature on the basis of the size of lesions: *tumores*, *tubercula*, and *pustulae*. The *phyma* fell into the first group (tumors), but it was actually described in the Galenic sense of a phlegmon involving the external lymph

glands (11e). Again, de Gorris (57) (1578) attributed a double meaning to the term *phyma*, defining it as a *tumor praeter naturam* and as a suppurative lesion in the Galenic sense. Sauvages (62b) (1763) made *phymata* the equivalent of what were essentially the "tumors against nature." This conception, although accepted by few authors, may have had a subtle effect on medical nomenclature. Thus, F. Hebra (76) (1866) regarded *phyma* as equivalent to tumor in a broad anatomic sense. Again, shortly before 1881, Hebra (77) coined the term *rhinophyma* for a variety of rosacea, in which irregular, lobulated swellings appeared on the nose. In this case Hebra used the word *phyma* in the sense of tumor or growth, without regard for its past historical meaning, and he specifically noted that the swellings in rhinophyma were not attended by suppuration. Even as late as 1948 the textbook on diseases of the skin by Ormsby and Montgomery (78) continued to define *phymata* as synonymous with tumors in the sense of new growths, benign or malignant (79).

One of the major causes that contributed to the use of *phyma* in a generic sense was the attempt on the part of translators and others to make this term synonymous with tubercle. In the classic Latin of Celsus *tuberculum* meant simply a swelling, and actually, the word was also employed to designate protuberances on the vertebrae (7h). During the Middle Ages, little seems to have been known about the medical meaning of *tuberculum*; for example, this was not one of the seven synonyms listed by Henri de Mondeville (42c, 43) as signifying a pathologic swelling. With the advent of the Renaissance the term *tuberculum* was reintroduced. Fernel's crude classification (11d, e) of the external lesions of the body had *tubercula* as a genus that included elevations of a certain size, but, curiously, *phyma* was not one of them. Botallus (56b) recorded gummas as tubercles. De Gorris (57), however, mentioned that *phyma* was a *tuberculum*. This terminology was followed in dictionaries published subsequently (80), in which the word *phyma* became equivalent to *tuberculum* or tubercle. In the flurry of Greek works that were made available during the nineteenth century, the translators often used these words synonymously. Even the learned Littré (81) followed this interpretation in his famous dictionary of the French language (1889). In the early part of the nineteenth century the tubercle became the crude gross anatomical unit of tuberculosis, and, as the Hippocratic writers had recorded *phymata* in the lungs, enthusiasts credited the ancient Greeks with more precise knowledge of tuberculosis of the lungs than the evidence warranted. Virchow (82) hotly disputed this exaggerated conclusion,

and, although some views advanced by this critic are no longer acceptable, historical data are generally in agreement with Virchow's thesis. Since then, the term tubercle has come to be defined in a minute anatomical sense (chiefly signifying tuberculosis), and this has created additional confusion. Dermatologic textbooks still define the tubercle as a nodule or solid lesion of a certain size, without regard to the cause; but in view of the possible confusion with lesions of tuberculosis, the term nodule is preferable by far.

IV.

Up to the beginning of the eighteenth century, relatively few new terms had been introduced into the nomenclature of swellings or tumors. One of the exceptions was the compound word *myxosarcoma*, which Severinus (13a) (1632) had coined to designate what seems to have been elephantiasis of the scrotum (110). In 1709 Littré recorded under the title of *lipome* a tumor composed of fatty tissue, which he differentiated from the *atherome*, *steatome*, and *meliceris* of old authors (mainly variants of sebaceous cysts and known to the French under the generic name of *loupes*). In 1791 J. P. Frank (84a) described vaguely a tumor on the chin, which, owing to its content of blood, he felt was best named as *haematoma*. So far as I can ascertain, this term was coined by Frank and was not representative of ancient usage, as claimed by Andrews. Up to the middle of the nineteenth century this expression was used for highly vascularized tumors (cavernous angioma, cancer, sarcoma), for swellings composed of blood (101c), and even for simple extravasations of blood. In formulating the word haematoma, Frank also indicated familiarity with more ancient terms ending in -oma, such as *atheroma*, *osteosteatoma*, *hygroma*, and *lipoma*; and undoubtedly he knew the word *carcinoma*. This observer (84b) (1793) likewise used the term *ecchymoma* in the modern sense of suggilation or ecchymosis. *Ecchymoma* and *ecchymosis* were considered as synonymous by the Hippocratic writers, and throughout the ages these expressions merely meant an extravasation of blood due to ruptured blood vessels, whether the resulting lesion was flat or raised. This was reflected in the usage to which the term *ecchymoma* was put during the eighteenth and even the early part of the nineteenth century when it meant an effusion of blood into the skin, with the production of flat or elevated areas (Sauvages (62c), 1763; Plenck (85), 1776; Alibert (86), 1817).

In the history of the ending -oma, the word *carcinoma* played a significant part toward the end of the eighteenth century. The relation of this

term to *carcinos* or the Latin *cancer* has been mentioned in the paragraphs devoted to a consideration of the Hippocratc writers, Celsus, and Galen. Thereafter the expression *carcinoma* was nearly always replaced by *carcinos* in the works of Byzantine authors, except possibly in the case of Nonnus (87) (tenth century). Again, with sporadic exceptions, the word *carcinoma* practically vanished from the Latin medical nomenclature of Western Europe starting from about the third or fourth century A. D. None of the translators of Arabic works used this term, nor can it be found in the writings of Western surgeons and physicians, such as Roger, Roland, William of Saliceto, Theodoric, Henri de Mondeville, Guy de Chauliac, Bernard de Gordon, Gilbertus Anglicus, John of Gaddesden, Johannes de Vigo, and the like. Apparently the first author to resurrect the term was Benivieni (70b) (1507). It is reasonable to ascribe this to the rediscovery of Celsus in the middle of the fifteenth century and the subsequent printing of his work. It is known, for example, that a copy of Celsus existed in the library of Benivieni (71). Aside from a few non-medical treatises in the original Greek, the bulk of Benivieni's library was composed of Arabic books with a few of Galen's works, all in Latin translation. This, together with some evidence that Benivieni was not too well acquainted with Greek terminology, would seem to support the belief that the resurrection of the term *carcinoma* was based on reading Celsus. During the Renaissance the original Greek treatises became available, and knowledge of that language increased steadily. Nearly all writers at that time understood that *carcinos* and *carcinoma* had been used in an equivalent sense by the Hippocratic writers and by Galen. However, the authors of the Renaissance preferred the Latin word *cancer*, which they employed in much the same way as had writers in the late Middle Ages, that is, as an *apostema* and as an ulceration. These processes had been differentiated by Celsus (7a) who had used the term *carcinoma* for the former and *cancer* for the latter. This distinction was not familiar to physicians and surgeons during the Middle Ages, nor was it accepted during the Renaissance, except possibly by Fallopius (72). The vast majority of writers therefore used the word *cancer* in this double sense, but gradually the term *carcinoma* entered the nomenclature again as a synonym for *cancer*. Bibliographies listing works published on this subject during the seventeenth and eighteenth centuries show that the term *carcinoma* was beginning to approach in frequency of use the expression *cancer*, and by the early part of the nineteenth century the word *carcinoma* was well intrenched as a synonym for *cancer*.

At this point medical nomenclature began to be influenced by several important events. First, dermatology emerged as a specialty under the leadership of Willan, Alibert, and their pupils; second, gross pathologic anatomy made tremendous strides under the stimulus of men such as Morgagni and Baillie. Many cutaneous diseases that had been formerly embraced within Galen's conception of the "tumors against nature" were now segregated by physicians interested in dermatology. New types of neoplasms, benign and malignant, were observed, classified, and demarcated on the basis of gross appearances. This, together with the beginnings of microscopic studies in the second quarter of the nineteenth century, made necessary a wide extension in medical nomenclature. In the course of this progressive increase in knowledge, the principle of formulating terms on the basis of analogy proved to be the determining factor in the coining of new words ending in -oma.

As early as 1803, Odier (89, 101d) described a tumor arising from nerves under the title of *neurome*. Hooper (98) (1828) was apparently the first to record *-ωμα* (-oma) as a "Greek final, which usually imports external protuberances." On this principle he formulated and explained the etymology of such terms as *osteoma*, *melanoma*, *cephaloma* (soft type of cancer), *lipoma*, *haematoma*, and *hygroma* (91). It may be noted that haematoma was used in the sense of a new growth (angioma) and that this was carefully differentiated from simple collections of extravasated blood. Thus, twenty-two years after Laennec (92) (1806) had described the melanoma under the heterogenous concept of melanosis, this tumor received its definitive name from Hooper. In 1836 Johannes Müller (93a) recorded the clinical features of enchondroma, and two years later described it more minutely in a publication that attracted wide attention. That many new terms were introduced on the basis of analogy with the word *carcinoma* is nowhere better shown than in Carswell's (94) magnificent atlas of pathologic anatomy (1838). Taking *carcinoma* as the prototype, Carswell subdivided it into the *scirrhoma* (hard) and *cephaloma* (soft) varieties, and, on the basis of special physical attributes, he added such variants as *melanoma*, *tyloma* and *lithoma*. Many terms used by Hooper and Carswell have since been abandoned; an exception was *melanoma*, a word that is still employed although often vaguely characterized.

In 1843 Hannover (95) introduced the conception of epithelioma, which he further elaborated in a treatise (1852) that was widely cited. The term *epithelioma* referred to benign epithelial growths that were

differentiated from true cancer owing to the absence of a specific cancer cell, the alleged failure to metastasize, and the non-occurrence of cachexia. How did Hannover formulate the expression *epithelioma*? The word *epithelis* (printed in later editions as *epithelia*) had been coined by Ruysch (96, 97) in the early part of the eighteenth century, and it had been formed from the Greek *ἐπί* (on) and *θηλή* (nipple). Under this term Ruysch designated that portion of the mucous membrane of the oral and gastrointestinal tract, which covered the nipple-like papillas; this was therefore analogous to the epidermis overlying the vascular and nervous papillas of the skin. To this word Hannover added the ending -oma to form *epithelioma*. In 1845 J. Henle (98) recorded a tubular type of cancer under the name of *siphonoma*. About the middle of the nineteenth century pathologists and clinicians began to refer to *papilloma*, *adenoma*, *fibroma*, *rhabdomyoma*, *angioma* (*angionoma*), *cylindroma*, *chloroma*, *lymphoma*, and the like.

At the same time the ending -oma was also used in a broader sense for lesions unrelated to neoplastic autonomous growths. For the cheesy or caseous deposits in lymph nodes and other structures of the body, Craigie (99) (1848) advocated the name *tyroma* (tyros, cheese). Swellings of inflammatory nature received the designation *phlogoma* (Küss, 1845) (100, 101e). Virchow (101f) introduced the concept of *granuloma* to describe swellings or tumors composed of granulation tissue and derivatives. In order to express the alleged histologic specificity of gummas, Wagner (102) (1863) coined the word *syphilom* (syphiloma). Still farther afield, Nordt (103) (1861) spoke of *sclerom* (scleroma) in reference to what is now universally recognized as diffuse progressive scleroderma. Hebra and Kohn (104) (1870) recorded the first examples of what they called *rhinosclerom*. Auspitz (105) coined the word *acanthoma* for a pathologic appearance in the skin, that corresponds for the most part to what is now called acanthosis. Many other manifestations of disease were thereafter labeled with expressions ending in -oma, the guiding principle being analogy.

The term *tumor*, which is so difficult to define, has had various arbitrary meanings assigned to it. This expression originally conveyed the simple conception of swelling in the classical Latin of Celsus. It was not used extensively until the Renaissance when it assumed the form of *tumores praeter naturam*, and later the qualifying phrase was dropped. During this entire period the word *tumor* merely designated all swellings, including diseases of the skin. In fact, so general was the meaning that Deidier

(68a) (1725) defined it as any manifestation elevated above the surface of the body (natural, non-natural, and against nature). In this terminology, which was patterned after Galen, a pregnant uterus or callus-formation in a fracture were designated as tumors, and remnants of this conception may still be found in the expression "splenic tumor" for enlargement of the spleen. However, in the period between the Renaissance and the beginning of the nineteenth century, the expression *tumor* had generally the connotation of a pathologic swelling (*praeter naturam*). In 1804 Abernethy (15) advised that the term should be applied only "to such swellings as arise from some new production, which made no part of the original composition of the body." It is interesting that, although Abernethy discussed chiefly what are now called malignant neoplasms, the gravid uterus was held to fall under this definition. As late as 1822 Boyer (107) had to stress that his own definition of tumor comprised internal as well as external elevations of pathologic nature. About the middle of the nineteenth century, owing to the stimulus of embryologic and minute anatomic studies, it came to be realized that neoplasms were derived from bodily constituents. Yet, Virchow's treatise on the pathologic tumors (*Die krankhaften Geschwülste*) was really more or less an elaboration of the Galenic conception of the "tumors against nature." For example, Virchow discussed extensively such lesions as warts, condylomata, gumma, lupus and scrofula. When this treatise is compared with a modern text, e. g. the one of Ewing (108) (1940), the fundamental difference between the two is found in the definition of the word *tumor*. Ewing restricts the meaning of this expression to that of an autonomous new growth of tissue. This specific scientific definition represents a distinct break with the past, although the conception itself has been subjected to criticism (109). On the other hand, the term *tumor* is still often used in a broad sense by physicians who deal with clinical entities that are not always easily and precisely recognized with the aid of the senses. Still others, such as the dermatologists, refer to *tumor* merely in the sense of a primary lesion that has a certain size and depth in the skin, without specifying the nature of the changes. However, the tendency has been to restrict the usage to neoplasms of autonomous nature, whether benign or malignant. To the laity, finally, this expression often means an affection requiring surgical or other drastic measures for cure, or a condition fraught with the possibility of grave consequences. It is therefore a word that must be used with considerable caution when conversing with lay people. We are still as sorely beset with difficulties in the precise definition of words as were the ancients.

From this survey it is clear that the ending -oma represents the modern clinicopathologic equivalent of the general expressions that were used throughout the centuries for the "tumors or swellings against nature." The historical data support the contention of those who have denied the existence of a Greek suffix -oma. On the other hand, the ending was not merely a corruption of the true Greek suffix -ma. When Hooper (90, 91) (1828) stated that -oma was "a Greek final, which usually imports external protuberances," he was in all likelihood thinking of the names of a number of tumors ending in -oma. It was not a conscious or unconscious attempt to corrupt the true Greek suffix -ma, but rather an error which had an air of great plausibility. Moreover, historical evidence shows that from the eighteenth century and more especially during the nineteenth century, words ending in -oma were fashioned chiefly on the basis of analogy. It is even possible that the same principle operated in the case of the ancients, but the data are too scant to warrant a more precise opinion.

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NOTES

1. Leider, M., and Rosenblum, M.: A philological note on a certain classical suffix commonly used in dermatologic and general medical nomenclature, *J. Invest. Dermat.* 9: 213, 1947. H. S. H. Vertue (*Guy's Hosp. Rep.* 96: 169, 1947) also concluded that -oma is a spurious Greek suffix, but, again, the historical data furnished in support of this thesis are incomplete.
2. Andrews, W.: *A history of scientific English*, 1947, p. 140.
3. Galen:
   (a) *Über die krankhaften Geschwülste* (*zwischen 169 und 180 n. Chr.*). Translated by P. Richter (Klassiker der Medizin no. 21), Leipzig, 1913.
   (b) Περὶ τῶν παρὰ φύσιν ὄγκων (De tumoribus praeter naturam) C. G. Kühn's Edition of Galen, Vol. 7, pp. 705-732.
4. Hippocrates: *Oeuvres complètes d'Hippocrate*. Translated by E. Littré, vols. 1-10, 1831-1861.
   (a) Aph. VI, 38 (vol. 4, p. 573); Epid. V, 20 (vol. 5, p. 221); Epid. V, 101 (vol. 5, p. 259); Epid. VII, 111 (vol. 5, p. 461); Dis. II, 37 (vol. 7, p. 53) etc.

(b) Hemorrhoids 4, 5 (vol. 6, pp. 441-3).
(c) In the Surgery 22 (vol. 3, p. 327); Fractures 11 (vol. 3, p. 459).
(d) Acute Dis. (Appendix) 1 (vol. 2, p. 399); Coan Prenotions, section 2, no. 394 (vol. 5, p. 673); see also C. Daremberg: *Oeuvres choisies d'Hippocrate*, second edition, 1855. Note 22, p. 259.
(e) Prognost. 7 (vol. 2, p. 127); Aph. VII, 59 (vol. 4, p. 597); Aph. 4, 34 vol. 4, p. 515); Epid. VII, 106 (vol. 5, p. 457).
(f) Epid. III, s3, 3 (vol. 3, p. 71); Aph. III, 20 (vol. 4, p. 495); Aph. III, 26 (vol. 4, p. 499); Aph. VII, 65 (vol. 4, p. 599); Aph. IV, 82 (vol. 4, p. 533); Dis. I, 19 (vol. 6, p. 173); Dis. II, 57 (vol. 7, p. 89); Prorrh. II, 11 (vol. 9, p. 33).
(g) Humors I (vol. 5, p. 479).
(h) Fractures 11 (vol. 3, p. 459).
(i) Epid. III, 7 (vol. 3, p. 85).

5. Galen: Edition of C. G. Kühn, 20 vols., 1821-1833.
(a) Hippocratis aphorismi et Galeni in eos commentarii, vol. 17, part 2, p. 877. See also definition of ὄγκος (oncos) in Thevenin (1691) and of οἴδημα (oedema) in de Gorris (1578) in references 17 and 52 resp.
(b) Definitiones medicae, no. 393, vol. 19, p. 443; De tumoribus praeter naturam, ch. xii, vol. 7, p. 726.
(c) Definitiones medicae, no. 370, vol. 19, p. 439.
(d) Hippocratis praedictionum Lib. I, Comm. III, vol. 16, p. 720.
(e) De tumoribus praeter naturam, vol. 7, pp. 731-2; also in German translation by P. Richter: *Über die krankhaften Geschwülste*, Leipzig, 1913, ch. xvii, pp. 25-6.
(f) De medendi methodo, Bk. II, ch. ix, vol. 11, p. 115; French translation by C. Daremberg, *Oeuvres anatomiques, physiologiques et médicales de Galien*, 1856, vol. 2, p. 786.

6. Herodotus: Bk. 3. 133 (translation by A. D. Godley, vol. 2, 1921, pp. 163-5).
7. Celsus: *De medicina*. Translated by W. G. Spenser, 3 vols., 1935-8. Bks. 1-4 in vol. 1; Bks. 5-6 in vol. 2 and Bks. 7-8 in vol. 3.
(a) Bk. V, 28. 2 (p. 129); (b) Bk. VII, 6 (p. 323); (c) Bk. V, 28. 3 (p. 133); (d) Bk. III, 10 (p. 273); (e) Bk. II, 1, 19 (p. 95); Bk. II, 7, 3 (p. 129); Bk. II. 8, 28 (p. 147); Bk. V, 18, 20 (p. 25); Bk. VI, 18, 2K (p. 275); Bk. VII. 6 (p. 323); (f) Bk. VII, 18, (p. 393); Bk. VII, 13 and 18 (p. 375, 393 seq.); (g) Bk. V, 28, 9 (p. 143); (h) Bk. VIII, 1, 13 (p. 483); (i) Bk. II, 1, 6 (p. 89); Bk. II, 7, 31; (j) Spencer's note a, vol. 1, p. 89.
8. The standard Greek dictionary by Liddell and Scott, 1925-1940, does not list any verb from which the term atheroma might be derived.
9. Alibert, J. L.: *Description des maladies de la peau observées à l'Hôpital Saint-Louis*, 1806. p. 113; Quelques recherches sur la chéloïde, *Mém. Soc. Med. d'Émulation*. 8: 744, 1817; *Monographie des dermatoses*. Second edition, vol. 2, 1835, p. 195.
10. Addison, T.: On the keloid of Alibert and on true keloid, *Med. Chir. Trans.*, second series, 19: 27, 1854.

11. Fernel, J.: *Medicina.* Physiologiae lib. VII, Pathologiae lib. VII, Therapeuticae lib. III. Paris, 1554.
(a) Pathologia Bk. VII, p. 224; (b) *ibid.* p. 230; (c) *ibid.* p. 219; (d) *ibid.* p. 220; (e) *ibid.* p. 221.
12. σύκωσις (sycosis)—Latin *ficus.* A rough fig-like excrescence on the flesh.
13. Severinus, M. A.: *De recondita abscessuum natura libri VII.* Naples, 1632.
(a) De novissime observatis abscessibus, p. 11.
(b) De abscessu critico, pp. 3, 20.
(c) De novissime observatis abscessibus, p. 83.
14. Lorry, A. C.: *Tractatus de morbis cutaneis.* Paris, 1777,
(a) p. 548; (b) p. 415.
15. Abernethy, J.: *Surgical observations.* London, 1804.
(a) p. 19; (b) p. 6.
16. Aretaeus: *The extant works of Aretaeus, the Cappadocian.* Translated by F. Adams, London, 1856. On the cure of chronic diseases, Bk. I, 2, p. 462 (for Greek text see p. 209).
17. Thevenin, F.: *Les oeuvres de Maître Francois Thevenin,* Lyon, 1691.
18. Hooper, R.: *Lexicon medicum*
(a) Fourth London edition, 1822; (b) Seventh London edition, 1841.
19. Adami, J. G.: *The Principles of Pathology,* vol. 1, 1908, p. 613.
20. *La Chirurgie d'Abulcasis.* Translated by L. Leclerc, Paris, 1861, ch. I, p. 53 (chapter heading); also ch. LIII.
21. Richter, P.: Über die spezielle Dermatologie des Ali ibn al-Abbas (Haly Abbas) aus dem 10. Jahrhundert unserer Zeitrechnung, *Arch. f. Dermat. u. Syph.* 113: 849, 1912.
22. Steinschneider, M.: Die griechischen Aerzte in arabischen Übersetzungen, *Virch. Archiv* 124: 288, 1891.
23. Constantinus Africanus: *Opera,* 2 vols., Basel, 1536 and 1539.
(a) p. 125; (b) p. 226.
24. Haly Abbas: *Liber totius medicinae.* Lyon, 1523.
25. (a) Rhazes: *Opera parva* 1510. Liber ad Almansorem decem tractatus continens cum nonnullis additionibus interlinearibus Gerardi Cremonensis. Tractatus septimus, chs. xi, xii, xiii, fol. CXV-CXVI.
(b) Avicenna: *Liber canonis totius medicinae.* Lyon, 1522. Bk. IIII. Fen III, ch. xv.
26. Rhazes: *Continens* . . . ordinatus et correctus per . . . Hieronymum Surianum, 1509. Bk. 27, fol. CCIC et seq.
27. An apostate is one who separates himself from, or forsakes faith, principles or party.
28. Galen: *Über die krankhaften Geschwülste (zwischen 169 und 180 n. Chr.).* Translated by P. Richter (Klassiker der Medizin, no. 21), Leipzig, 1913.
(a) ch. iii, p. 14; (b) ch. xv, p. 24; (c) ch. xv, p. 23.
29. Oribasius: *Oeuvres d'Oribase.* Translated by Bussemaker and C. Daremberg.
(a) Bk. XLIV, 6 (vol. 3, p. 564); (b) vol. 5, p. 372.
30. *The seven books of Paulus Aegineta.* Translated by F. Adams, vol. 2, 1846.
(a) Bk. IV, ch. xxii, p. 71; (b) Bk. IV, ch. xviii, p. 54.
31. In translating these and other sections of Paulus Aegineta, Adams used

the word abscess. Whereas I have been unable to compare the original Greek text of Bk. IV, there is little doubt in my mind that Adams' term abscess was employed for *apostema*. This is all the more likely since in comparing the Greek text of Bk. VI (R. Brian: *Chirurgie de Paul d'Egine*, 1855), only the Greek term apostema is used in the aforementioned sense.

32. Pliny the Elder: *Naturalis historiae libri XXXVII*. Edition of G. Sillig, Hamburg, 1853. Lib. XX, cap. iii, 16 (vol. 3, 292).
33. Cassius Felix: *De medicina ex graecis logicae sectae auctoribus*. Edition of V. Rose, Leipzig, 1879. Cap. xviii, p. 25; cap. xxi, p. 32.
34. Caelius Aurelianus: *De morbis acutis et chronicis libri VII*, Amsterdam, 1709. Bk. II, ch. xiv, 93, p. 113.
35. Practica Petrocelli Salernitani, cap. v, in *Collectio Salernitana* (S. De Renzi), vol. 4, p. 227.
36. Gariopontus: *Passionarius Galeni*. Lyon, 1526. Bk. V, ch. xiiii, fol. LXXVII r.
37. Celsus has been snubbed by some medical historians because he was probably not a physician in a strict sense. It has been pointed out, for example, that he was not cited by Galen. However, on historical grounds, irrespective of the source of his writings, Celsus looms as an influential figure in the history of many branches of medicine, and especially in that of dermatology. As will be shown in the sequel, he was widely read at the end of the fifteenth century and during the sixteenth century when his influence on medicine became manifest.
38. *Glossulae quatuor magistrorum super chirurgiam Rogerii et Rolandi*. Edition of C. Daremberg, Paris, 1854, p. 81; Chirurgia de Ruggiero, cap. xxii, in *Collectio Salernitana* (S. de Renzi), vol. 2, 1853, p. 478.
39. Theodoricus in: Guy de Chauliac. *Chirurgia* etc. Venice, 1498.
(a) Bk. III, ch. v, fol. 126 r.; (b) Bk. III, ch. vii, fol. 127 r.
40. Bruno in: Guy de Chauliac. *Chirurgia* etc. Venice, 1498. Bk. II, ch. v, fol. 97 r.
41. Lanfranc: Chirurgia parva in: Guy de Chauliac. *Chirurgia* etc. Venice, 1498. Ch. x, fol. 162 r.
42. Henri de Mondeville: *Chirurgie*. Translated by E. Nicaise, Paris, 1893.
(a) pp. 656, 477, 717, 704; (b) pp. 468, 477, 704; (c) p. 669.
43. *Die Chirurgie des Heinrich von Mondeville*. Edition of J. L. Pagel, Berlin, 1892. Tract III, Doctrine II, ch. 2, p. 458.
44. Yperman, J.: *La chirurgie de maître Jehan Yperman*, Anvers, 1863. Apostema is used throughout the book; for example, p. 113.
45. Guy de Chauliac: *La grande chirurgie*. Translated by E. Nicaise, Paris, 1890.
(a) Second Treatise, p. 75 et seq.; (b) pp. 136, 316.
46. Gilbertus Anglicus: *Compendium medicinae*. Lyon, 1510. Bk. VII, fol. CCCXXIX v.
47. John of Gaddesden: *Rosa anglica practica medicine a capite ad pedes*. Edited by Nicolo Scillacio, 1492, fol. 156.
48. Bernard de Gordon: *Omnium aegritudinum a vertice ad calcem . . . quod Lilium medicinae appellatur*. Paris, 1542. Rubrica 8, fol. 41 v; cap. 19, fol. 35 r.

49. Johannes de Vigo:
    (a) *Practica in arte chirurgica copiosa.* Rome, 1514. Bk. II, fol. 10 et seq.
    (b) *The most excelent worckes of chirurgery, made and set forth by Maister John Vigon head chirurgien of oure tyme in Italy.* London, 1550. Bk. II, Treatise I.
50. During the sixteenth and seventeenth centuries and beyond, the English used the terms apostumes or impostumes (often also spelled aposthumes or imposthumes).
    (a) *The workes of that famous chirurgion Ambrose Parey.* Translated from Latin into English by T. Johnson, London, 1634. Bk. VII, ch. i, p. 249;
    (b) Blancard, S.: *The physical dictionary.* Seventh edition, London,1726.
51. Tagaultius, J.: *De chirurgica institutione libri quinque, his accessit sextus liber de materia chirurgica, authore Jacobo Hollerio Stempano.* Paris, 1543.
    (a) p. 1 et seq.; (b) p. 7. A French translation of this work is given in, Tagaultius, J.: *La chirurgie de . . .* Lyon, 1580.
    (a) p. 1 et seq.; (b) p. 11.
52. Leone, Domenico: *Methodus curandi febres, tumoresque praeter naturam, ex Graecorum placitis deprompta.* Bologna, 1562, p. 97.
53. Borgarucci, Prospero: *Andreae Vessalii . . . chirurgia magna in septem libros digesta.* Venice, 1569. Bk. V, fol. 265 r.
54. Paré, A.: *Oeuvres,* Paris, 1575. Bk. I, ch. i, p. 228.
55. The gummas were unknown to physicians prior to the end of the fifteenth century. Their widespread occurrence in the early part of the sixteenth century affords a powerful, indeed irresistible proof in my opinion, that syphilis first appeared in Europe at the end of the fifteenth century.
56. Luisinus, A.: *De morbo gallico omnia quae extant.* Venice, 1567.
    (a) See N. Massa: vol. 1, p. 40; G. Fallopius: vol. 1, p. 716.
    (b) L. Botallus: Appendix vol. 2, ch. xv, p. 26.
57. de Gorris, J.: *Definitionum medicarum, libri xxiiii,* Frankfurt a. M., 1578.
58. Joubert, L.: *Annotations . . . sur toute la chirurgie de M. Guy de Chauliac.* Lyon, 1642, p. 281 et seq.
59. Castelli, B.: *Lexicon medicum Graeco-Latinum,* Geneva, 1746.
60. Wiseman, P.: *Several chirurgical treatises.* Second London edition, 1686.
    (a) Treatise I, of tumours; (b) ch. iv, p. 45.
61. Astruc, J.: *Traité des tumeurs et des ulcères,* 1759, vol. 1, ch. ii, p. 36.
62. Sauvages, F. Boissier de: *Nosologia methodica,* Amsterdam, 1763.
    (a) vol. 2, p. 72; (b) vol. 2, p. 25; (c) vol. 2, p. 14.
63. Colin, G.: *Avenzoar: sa vie et ses oeuvres,* Paris, 1911. Colin (p. 95), citing the translation by "Paravices" (c. 1280), expressed surprise that the word apostema was used in the sense of swelling (against nature) since the expression meant "abcès." However, the data recorded in my article show that during the period in question the term apostema had taken on a broader meaning.
64. A. R. Mills, Peasant Remedies from the Greek Islands, *Bull. Hist. Med.* 22: 441, 1948, has reported that among the peasants in the Aegean Islands,

who "have lived almost untouched and unchanged for centuries," the word *apostema* is still used in the Hippocratic sense of abscess. I take it that this means a suppurative process.

65. Hieronymus Crassus: *Chirurgi de tumoribus praeter naturam.* Venice, 1562.
66. Fuchs, Leonhard: *Institutionum medicinae libri quinque.* Basel, 1583.
    (a) Bk. III, s. 1, ch. xxvii, p. 630; (b) ibid. p. 637.
67. Fabricius ab Aquapendente, H.:
    (a) *Pentateuchos cheirurgicum . . . publicis in lectionibus ab auctore propositum . . .* opera Johannis Hartmanni Beyeri . . . Frankfurt, 1592
    (b) *Opera chirurgica.* Venice, 1619, vol. 1, p. 42.
    (c) *Medicina practica . . .* opera Petri Bourdelotii, Paris, 1634.
    (d) *Oeuvres chirurgicales de Hierosme Fabrico d'Aquapendente.* Lyon, 1674.
68. Deidier, A.: *Deux dissertations médicinales et chirurgicales.* Paris, 1725.
    (a) p. 105; (b) p. 112.
69. Stillwell, M.: *Incunabula in American libraries,* 1940, p. 138.
70. Antonii Benivienii *De abditis nonnullis ac mirandis morborum et sanationum causis.* Florence, 1507.
    (a) cap. i; (b) cap. lxxii.
71. de Vecchi, B.: I libri di un medico umanista fiorentino del sec. XV. Dai "Ricordi" de maestro Antonio Benivieni, *La Bibliofilia* 34: 293, 1932. This article was printed in French in *Janus* 37: 97, 1933.
72. Fallopius, G.: *Libelli duo, alter der ulceribus: alter de tumoribus praeter naturam.* Second edition, Venice, 1566.
73. Fracastorius, H.: *De contagione et contagiosis morbis et eorum curatione, libri III.* Translated by W. C. Wright, 1930.
    (a) ch. xv, p. 177.
74. David: Mémoire sur les abscès. *Mém. sur les sujets proposés pour le prix de l'acad. roy. de chir.* 4: 119, 1778.
75. Bateman, T.: *A practical synopsis of cutaneous diseases.* Third edition, London, 1814, p. 270.
76. Hebra, F.: *On diseases of the skin,* London, 1866, vol. 1. Translation of C. Hilton Fagge, p. 12.
77. Hebra, F. and Elfinger, A.: *Atlas der Hautkrankheiten.* Vienna, 1856-1876. Heft VII, Plate 6.
78. Ormsby, O. and Montgomery, H.: *Diseases of the skin.* Seventh edition, 1948, p. 75.
79. Since the term phyma has been used here in an erroneous sense, without due regard for its historical meaning, and since the expression has more or less fallen into oblivion, it should be dropped altogether from dermatologic text-books. There still remains the "synonymous" word tumor, which, it must be understood, has in this restricted dermatologic sense only a gross anatomic, and not a pathologic connotation. The modern dictionary meaning of phyma should be revised to read: a variety of tumor (swelling), generally but not always used in the past for a suppurative process.
80. Blancard, S.: See reference no. 50b; Hooper, R.: see reference no. 18; Mayne, R. G.: *An expository lexicon.* London, 1860.

81. Littré, E.: *Dictionnaire de la langue française*, 1889.
82. Virchow, R.: Phymatie, Tuberculose und Granulie, *Virch. Arch.* 34: 11, 1865.
83. Littré: Diverses observations anatomiques. *Hist. de l'Acad. roy. des Sci.* 1709, p. 28.
84. Frank, J. P.:
    (a) *Delectus opusculorum medicorum*, vol. 3, 1791, p. 234 et seq.
    (b) *De curandis hominum morbis*, vol. 4, 1793, p. 97.
85. Plenck, J. J.: *Doctrina de morbis cutaneis.* Vienna, 1776, p. 26.
86. Alibert, J. L.: *Nosologie naturelle*, vol. 1, 1817, p. 349.
87. Nonnus: *De omnium particularium morborum, curatione, sic ut febres quoque et tumores praeter naturam complectitur.* Strasbourg, 1568, cap. lxx.
88. Wolff, J. J.: *Die Lehre von der Krebskrankheit.* Second edition, Jena, 1929, pp. 119-122.
89. Odier's work was not accessible to me but was cited by R. Virchow (see reference no. 101 d), and there are direct citations in W. Wood: Observations on painful subcutaneous tubercle, *Trans. Med. Chir. Soc. Edinb.*, 3: 317, 1829; R. W. Smith: *A treatise on the pathology, diagnosis and treatment of neuroma.* Dublin, 1849.
90. Hooper, R.: *The morbid anatomy of the human brain.* London. 1828.
91. That Hooper was responsible at this time for the introduction of -oma as a Greek suffix is shown further by the absence of reference to this point in the 1822 edition of his dictionary and by the inclusion of this point in the edition of 1841 (see reference no. 18). This was probably the original source from which modern dictionaries have continued to list such a suffix.
92. Laennec, R. T. H.: Sur les melanoses, *Bull. de l'École de Méd. de Paris*, Jan. 2, 1806, no. 2, p. 24.
93. Müller, Johannes:
    (a) Jahresbericht über die Fortschritte der anatomisch-pathologischen Wissenschaften im Jahr 1835, *Arch. f. Anat., Physiol., u. wissensch. Med.* 1836, p. ccxx in introductory article of issue.
    (b) *Über den feineren Bau und die Formen der krankhaften Geschwülste.* Berlin, 1838. An English translation by C. West was issued in 1840.
94. Carswell, R.: *Pathological anatomy.* Illustrations of the elementary forms of disease. London, 1838.
95. Hannover, A.: *Das Epithelioma, eine eigenthümliche Geschwulst die man im allgemeinen bisher als Krebs angesehen hat.* Leipzig, 1852, p. 7.
96. Ruysch, F.: *Thesaurus anatomicus.* Amsterdam, 1727, VII, no. 40, p. 11-12.
97. Virchow, R.: Ueber die Orthographie des Wortes "Epithel," *Virch. Arch.* 11: 465, 1857.
98. Henle, J.: Rohrengeschwulst-Siphonoma, eine neue Art pathologischer Geschwülste, *Ztschr. f. rationelle Med.* 3: 130, 1845.
99. Craigie, D.: *Elements of general and pathological anatomy.* Second Edinb. edition, 1848, p. 222.
100. Küss, E.: *De la vascularité et de l'inflammation.* Strasbourg, 1846, p. 49. (this reference was not available to me but was cited by Virchow, see reference no. 101 e).

101. Virchow, R.: *Pathologie des tumeurs.* Translated into French by P. Aronssohn, vol. 1, 1867; vol. 3, 1871; vol. 2, 1869.
(a) vol. 1, p. 15; (b) vol. 3, p. 77 seq.; (c) vol. 1, p. 125; (d) vol. 3, p. 425; (e) vol. 1, p. 2; (f) vol. 2, p. 381 seq.
102. Wagner, E.: Das Syphilom, *Arch. d. Heilk.* 4: 1, 1863.
103. Nordt, J.: *Uber das einfache Sclerom der Haut.* Inaug. Diss. Giessen, 1861.
104. Hebra, F. and Kohn, M.: Ueber ein eigenthümliches Neugebild an der Nase-Rhinosclerom, *Wien. Med. Wchschr.* 20: 1, 1870.
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THE DEATH OF PRESIDENT GARFIELD

STEWART A. FISH*

On the morning of July 2, 1881, President James A. Garfield was injured fatally by a bullet fired from the gun of an insane killer. He did not succumb to the effects of the wound for twelve weeks. The details of his long illness and its final outcome are of significant historical interest.

Probably few other surgical cases have been followed more closely, by so many people, and subjected to a greater amount of criticism from the medical profession and the laity, than that of President Garfield. The concern produced by the wounding of the President brought forth an untold number of editorials in newspapers, medical journals and periodicals. These dealt with the progress of the case, and even with the question of malpractice on the part of the attending surgeons. One of the principal consultants in the President's case was Dr. D. Hayes Agnew, a former Professor of Surgery at the Univerity of Pennsylvania, a surgeon of great renown, and a figure whose name, even to this day, is well known to all who have been associated with the University of Pennsylvania School of Medicine.

In 1881, just 69 years ago, the theory of antisepsis was still being debated within the medical profession, and although Dr. Agnew was one of the strongest supporters of this theory, he arrived at the President's bedside too late to prevent serious infection of Garfield's wound from the unwashed, examining fingers of preceding surgeons who were not yet convinced of "Listerism's" efficacy.

It is interesting to note here the reluctance with which Lister's theory of antisepsis was accepted in America. Lister first employed his valuable technic in a case of compound fracture in August 1865, with complete success. Two years later, in 1867, he published a comprehensive and convincing summary of two years' work. The skepticism which greeted this famous contribution to medicine is a familiar story and need not be repeated here. However, as an example of the complete indifference with which some of the leaders of American surgery accepted this theory, Dr. S. D. Gross, Professor of Surgery in the Jefferson Medical School in 1876, said: "Little, if any faith, is placed by any enlightened or experienced surgeon on this side of the Atlantic in the so-called carbolic acid

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treatment of Professor Lister, apart from the care which is taken in applying the dressing; or what is the same thing, in clearing away clots and excluding air from the wound; an object as readily attained by the oil dressing---composed of a thin layer of cotton or patent lint, wet with olive oil—which I have myself employed for many years with signal benefit, in nearly all cases of wounds under my charge, whether the result of accident or design."

Thus, at the time of Garfield's wounding, 16 years after the introduction of the fundamental surgical principle of antisepsis, the theory was still not generally accepted or taught in America. It is understandable that Garfield's case terminated in such an unhappy fashion, and perhaps the final outcome would have been entirely different, if the attending surgeons had adhered to Lister's principles from the moment of injury.

The x-ray had not yet been discovered, so there was no accurate method of locating the lead pistol ball which had struck the President. Several scientists and inventors had developed electrical devices which, supposedly, were capable of locating the bullet, but proved to be useless* One of these machines was offered to the attending surgeons by a dentist who testified to its efficiency by stating, " My first experiment was on my own person having had the misfortune to receive a full charge of shot in the rear, from a friend's gun whilst woodcock shooting."

Anesthetic and surgical technics were progressing rapidly in 1881, but they were not yet sufficiently advanced to enable the surgeons to attempt the formidable exploratory operation that would have been necessary to locate the missile.

Of course the public was incensed greatly over the attempt upon the President's life, and the question of the assassin's sanity was a subject of heated debate within medical circles. Sociological implications arose in the discussion over the penalty the murderer should pay, and several legislative acts were suggested to curb the criminally insane.

The nature and management of the President's wound, the complications that arose, and the final outcome of the case are of interest from a strictly medical standpoint, and are recounted in some detail.

This is the report of a single surgical case, but it gives an excellent view of the status of medicine and surgery in the United States in 1881. The best the profession had to offer was not enough to save the patient.

And now for the story.

*[See the editorial note on " Simon Newcomb and the Location of President Garfield's Bullet " in this issue of the *Bulletin*, pp. 393-397—Ed.]

On the hot summer morning of July 2, 1881, Charles J. Guiteau, a bearded, sparsely built, forty-year-old man, ate a hearty breakfast at the Riggs House in Washington, D. C. He rose from his meal, left without paying, and walked hurriedly to a secluded spot at the foot of 17th Street on the mud flats of the Potomac River. There he removed a revolver from his jacket, placed a stick in the wet mud, and fired several practice shots, using the stick as a target. He seemed pleased with himself, replaced the pistol in his pocket, and made his way to the old Baltimore and Potomac Railroad station at 6th and B Streets. Guiteau looked at his watch—it was 8:30 a. m. He loitered about the entrance to the station, had his boots blacked, engaged a carriage to wait for him, and then went inside the building and stood quietly in a corner.

At twenty minutes past nine, the President of the United States, James A. Garfield, and the Secretary of State, James G. Blaine, entered the station on their way to board a train which would take them to Williamstown, Massachusetts, where Garfield was to attend graduation exercises at Williams College, his alma mater.

The bearded stranger stepped through the small crowd, leveled his pistol at the President's side, and fired two shots directly at the President. He turned and walked calmly toward the door, but before he could make his escape, he was seized by several horrified onlookers. He did not resist and exclaimed in a somewhat excited voice, "I shot him purely as a political necessity under Divine Pressure." Thus did Charles J. Guiteau fatally wound the twentieth President of the United States.

Of the two shots which Guiteau fired at Garfield from his British Bull-Dog, 44 caliber pistol, one grazed his right arm harmlessly, but the second one struck him in his right side at the level of the eleventh rib. The President fell immediately to the floor, sinking down on his wounded side. He was carried at once to an office room on the second floor of the station. He vomited several times, but remained conscious.

Numerous doctors, hurriedly summoned by different persons, soon arrived at the President's couch. Among them was Dr. D. W. Bliss, the President's personal physician, who had been notified by the Secretary of War, and who immediately took charge.

The President was removed at once to the White House, where it was apparent that he was in shock for he was pale, his pulse was rapid and weak, and his extremities were cold. There was very little external bleeding, and it was believed he was dying from internal hemorrhage. The

President complained of pain in his ankles, and in the right side of his scrotum, but there was never any loss of motor function of the lower extremities. " Morphia " was administered and examination of the wound was made.

Surgeon General Wales of the Navy introduced his finger to its full extent into the wound and declared that the liver was perforated. Dr. J. J. Woodward introduced his finger sufficiently deeply to determine that the eleventh rib was broken. Dr. Bliss passed several probes but made no comment. Other physicians examined the President including a homeopathic physician who rushed into the room, turned Garfield on his side, put his fingers deeply into the wound and exclaimed, " My God, General, you ought to have surgical advice! " to which President Garfield replied, " There are about forty of them in the adjoining room—go and consult with them." This doubtless showed the impression left upon the President's mind by the examinations to which he had been subjected. However, at a later date, attending physicians maintained that the wound had not been probed unduly and that, " Throughout the whole course of treatment, so far as it was possible to apply the system of antiseptic surgery, advocated by M. Lister, to a wound of this character, it was rigorously employed."

It was the general opinion that the President had not long to live, and he was told so—a fact which he accepted with great equanimity. The President's wife arrived at the White House late in the afternoon of the same day, from Elberon, New Jersey, where she had been recuperating from a severe attack of malaria. The patient was sedated with frequent doses of " morphia " and as night approached, he was resting peacefully despite the fact that he was considered to be dying. So passed the first day.

The President slept fitfully during the night with the aid of the narcotic which he was receiving, and on July 3, the attending physicians all agreed that the wound was not as serious as had previously been thought. Nevertheless, little hope was still held for his survival. In the evening of July 3, Garfield's temperature began to rise; his abdomen became distended, and he complained of severe pains in his feet, which he referred to as " tiger claws." Bulletins on the President's condition were now being released several times a day from the Executive Mansion, and the entire United States was following the case with great interest.

On the Fourth of July, Dr. D. Hayes Agnew of Philadelphia made his appearance upon the surgical scene, along with Dr. Frank H. Hamilton of

New York. Dr. Agnew was at this time the outstanding surgeon in the United States. He and Dr. Hamilton had been summoned on the recommendation of Dr. Bliss and the other attending surgeons. They consulted with the other physicians at the White House and probed and examined the wound thoroughly. They both agreed that the "course of treatment" was correct. Dr. Agnew told Secretary of State Blaine that the outlook was very gloomy, and the prospects of locating the pistol ball were not good. Agnew and Hamilton then left Washington with the understanding that they would be notified immediately in case of any sudden change in the President's condition.

On July 5, seventy-two hours after the injury, the President vomited occasionally, (which was probably due to the large doses of morphine he had received) but his distention was somewhat diminished. By this time, the leg pain had almost disappeared, and oral feedings with chicken broth were begun. His temperature was 100.5 degrees and his general condition was surprisingly good. The President had his first normal bowel movement on July 6. It was agreed at this time that the ball had not penetrated the abdominal cavity—an event which had been feared since his wounding.

On July 8, the morphine was reduced to a single dose of one-quarter grain in the evening to induce sleep. On the same day the wound began to show the first evidence of discharging "laudable pus." A "stimulant" was given to the President in the form of some "old and excellent rum." Garfield's temperature now began to rise and fall irregularly, and the physicians started administering five grains of quinine sulfate each day, in the belief that he was suffering from malaria (a disease which was prevalent in Washington at that time), apparently not considering the wound infection as a possible cause of the intermittent fever.

Four days later, on July 12, the President's condition was favorable. The wound, which was discharging "healthy pus," was dressed "antiseptically" with carbolic acid twice a day. The irregular temperature continued, so the antimalarial dose of quinine was doubled.

From July 13 to July 21, the condition of the President remained unchanged. His diet was increased, and some solid foods were well tolerated.

On July 22, Dr. Bliss, while dressing the wound, noticed some spicules of bone, and fragments of clothing, in the discharging pus. About noon on the 23rd, the President vomited. This was immediately followed by a severe chill. That night his temperature rose to 104 degrees, and his condition became critical. Agnew and Hamilton were summoned immediately, and a special train brought them with great speed to Washington. On the

morning of July 24, Agnew made a drainage incision three inches below the wound. "The President endured the knife without ether and without the slightest tremor."

A circumscribed area of induration had appeared in the right groin on the previous day and at this time Agnew succeeded in passing an irrigating catheter through the incision a distance of seven inches toward the groin. Since the course and location of the pistol ball were still unknown, the track followed by the catheter was now considered to be the true course of the missile. The President's condition improved gradually after the drainage incision had been made, until on July 31, his condition was "excellent." He "slept sweetly" during the night, ate well, but his temperature was still irregular and the wound continued to discharge large amounts of "laudable pus" from its two openings.

The "morphia" was withdrawn completely on August 5, and on the same day the surgeons discontinued the administration of the quinine after deciding that Garfield's irregular fever was not due to malaria.

On August 12, the surgeons, in consultation, decided to perform further surgical drainage, so the President was "etherized" and the previous incision was extended three inches downward and forward. A counter opening was now made into the suspected track of the ball, just below the margin of the 12th rib. The President withstood the procedure well, and the drainage incision began to discharge purulent material the next day. The septic fever continued, but was of lesser degree.

On the morning of August 15, the President vomited his breakfast, and continued to vomit throughout the day. All food by mouth was stopped, and feedings by rectum were instituted. This complication was not unexpected, as Garfield had previously been known to have an unstable gastrointestinal system, and the ether anesthesia probably caused enough irritation to upset his stomach. Proctoclyses were continued with favorable results, and the vomiting began to decrease on August 17.

The next day an additional complication appeared. The right parotid gland became swollen and painful, which was probably due to poor oral hygiene and to dehydration.

On August 20, nutritive enemata were still being given, but at longer intervals, and oral feedings were gradually being increased. The parotid gland remained swollen, but was now painless. The bullet wound was irrigated by means of the catheter, which now passed 12½ inches into the suspected track of the pistol ball.

On the afternoon of August 21, the President vomited a number of

times, so all oral feedings were discontinued, and nutritive enemata were re-instituted. The parotid swelling had not changed materially, except that it began to cause " an annoying accumulation of viscid mucous in the back of the mouth." A complete right facial paralysis (Bell's palsy) developed at this time, which was due to the inflamed and swollen parotid gland pressing on the facial nerve. On August 22 and 23 the President's gastric symptoms improved. Small feedings were begun again, but the mucous drainage from the parotid increased, and it was feared that the gland would suppurate.

On August 24, Dr. Agnew and Dr. Hamilton were summoned from Philadelphia by Dr. Bliss to care for the parotid swelling, and to give their opinion upon the advisability of removing the President from Washington to some more favorable climate.

Shortly after noon on the 24th, the parotid swelling was incised by Dr. Hamilton, without anesthesia, and a small amount of pus liberated. The next day the President's gastric intolerance was entirely gone, and sufficient liquids could be given orally to maintain his nutrition. The parotid swelling had not noticeably diminished, and there had been a rise in his temperature since the incision and drainage the day before. The surgeons agreed at this time that the President was in no condition to be moved from the White House, but that he should be removed at the first favorable opportunity.

Secretary Blaine's message to Lowell (Minister to London) was gloomy that day, and the President's condition was considered very poor. For the first time in the long course of his illness, Garfield's consciousness became clouded and his mind began to wander. He muttered in his sleep and could not be fully aroused from his lethargy. It was estimated that the President had lost " 80 pounds of flesh," which was two-fifths of his body weight. Grave concern was held for his survival. On August 26, there was more discharge from the parotid incision, and pus began to drain from the right external auditory meatus, through two sinuses that appeared spontaneously in the internal canal. There was some discussion as to whether this drainage from the ear was from the parotid suppuration, or whether it was due to an acute otitis media. No definite diagnosis was ever made. Liquid oral feedings were well tolerated, and again occasional nutritive enemata containing stimulants were given. With this treatment, the President's mind began to clear slowly and his general condition began to improve. Several additional discharging sinuses appeared just below the right ear on August 28. The gunshot wound was progressingly favorably, and

continued to drain quantities of "healthy looking pus." The slow improvement of Garfield's condition continued in spite of the parotid suppuration, and it appeared the President had successfully passed through another crisis.

Another incision and drainage of the parotid gland were performed on August 30, and on the next day the swelling of the gland was noticeably less. A spontaneous opening from the parotid abscess into Garfield's mouth occurred on the thirty-first, and irrigating fluid passed from the abscess into the President's throat.

On September 2, two months after the injury, the hottest weather of the summer descended upon Washington, and the President became restless from the extreme heat. Not a leaf could be seen to stir upon the trees surrounding the White House, and Blaine's message to Lowell on September 2 stated that, "Even in the September climate of Washington such an oppressive day as this has been, is rare." The patient's temperature rose, and much fluid was lost by perspiration. The parotid abscess was greatly improved and food was taken by mouth, "with relish."

Plans had now been completed to move the President at the first opportunity to his summer home near Long Branch, New Jersey, in order to "escape the malarious influence of the Washington climate at this season." The unseasonable heat wave increased in severity, and September 5 was reported as the hottest day of the year in Washington.

On September 6, the President was removed from the White House in a specially designed bed, and transported without incident, on a train furnished by the Pennsylvania Railroad, to Francklyn Cottage, the President's summer home, at Elberon, New Jersey. He withstood the journey well, but the extreme heat of the season enveloped the entire eastern seaboard, and little relief was experienced at the seaside cottage at Elberon. Despite the fact that September 7 was reported as being the hottest day ever recorded at nearby Long Branch, New Jersey, the President expressed himself as "pleased and even delighted with the change."

On September 10, the parotid swelling had almost entirely disappeared; there was no gastrointestinal disturbance, and the gunshot wound was progressing satisfactorily. A large slough of connective tissue was removed from the parotid area the same day.

The President had had a mild bronchitis since August 25, and the feared complication of bronchopneumonia suddenly appeared on September 10, along with a hacking productive cough. The heat wave disappeared the same morning, and a cold easterly storm blew in from the sea. The

President's general condition remained fair, however, and he sat up in a chair for half an hour on the eleventh and twelfth. The cough and purulent expectoration continued. The gunshot wound began to drain smaller amounts, and the discharge appeared "less healthy." The temperature continued to be irregular but was generally lower. From September 12 to 17 the President's condition did not change. The attending physicians began to grow more confident that the President might eventually recover, since his condition was not deteriorating. However, this was the calm before the storm.

During the afternoon of September 17, Garfield had a severe chill, his pulse and temperature rose, and all the hopes of the previous days were destroyed. A profound weakness overcame the President. On September 18, the cough became troublesome and his temperature began to be extremely irregular. Rigors followed by fever appeared, and his condition was considered very grave. On the afternoon of September 19 his pulse rose to 120 and became weak; his temperature rose to 102 degrees. The chills continued throughout the day. The President took some nourishment late in the evening and fell into a restless sleep. At 10:10 p. m. he awoke suddenly and cried out, "Oh, how it hurts here," indicating an area just over his heart. He fell back onto his pillow and almost immediately became comatose. Mrs. Garfield and the physicians came immediately to the President's bedside. His respirations became weaker, and at 10:35 p. m. he ceased breathing, and could not be revived. Dr. Bliss placed his ear on the President's chest and heard the last few flickers of life in Garfield's heart.

Thus, after twelve long weeks of suffering, and the best care that American medicine and surgery could offer, the President was dead.

The dawn of September 20 was cloudy and cold, and by mid-morning the entire United States knew of the President's sudden but not completely unexpected death. By 4:30 in the afternoon, 18 hours after the death of the President, the original six attending surgeons had gathered at the Cottage along with Dr. Andrew Smith of Elberon and Dr. D. S. Lamb of the Army Medical Museum. An autopsy was performed, and at last the hiding place of the pistol ball was discovered, and the cause of death was revealed to all.

The external examination showed the body to be somewhat emaciated. There were several bed sores over the sacral area and the site of the bullet wound was well healed. Upon opening the abdomen, the physicians found that the omentum and transverse colon were adherent to the liver

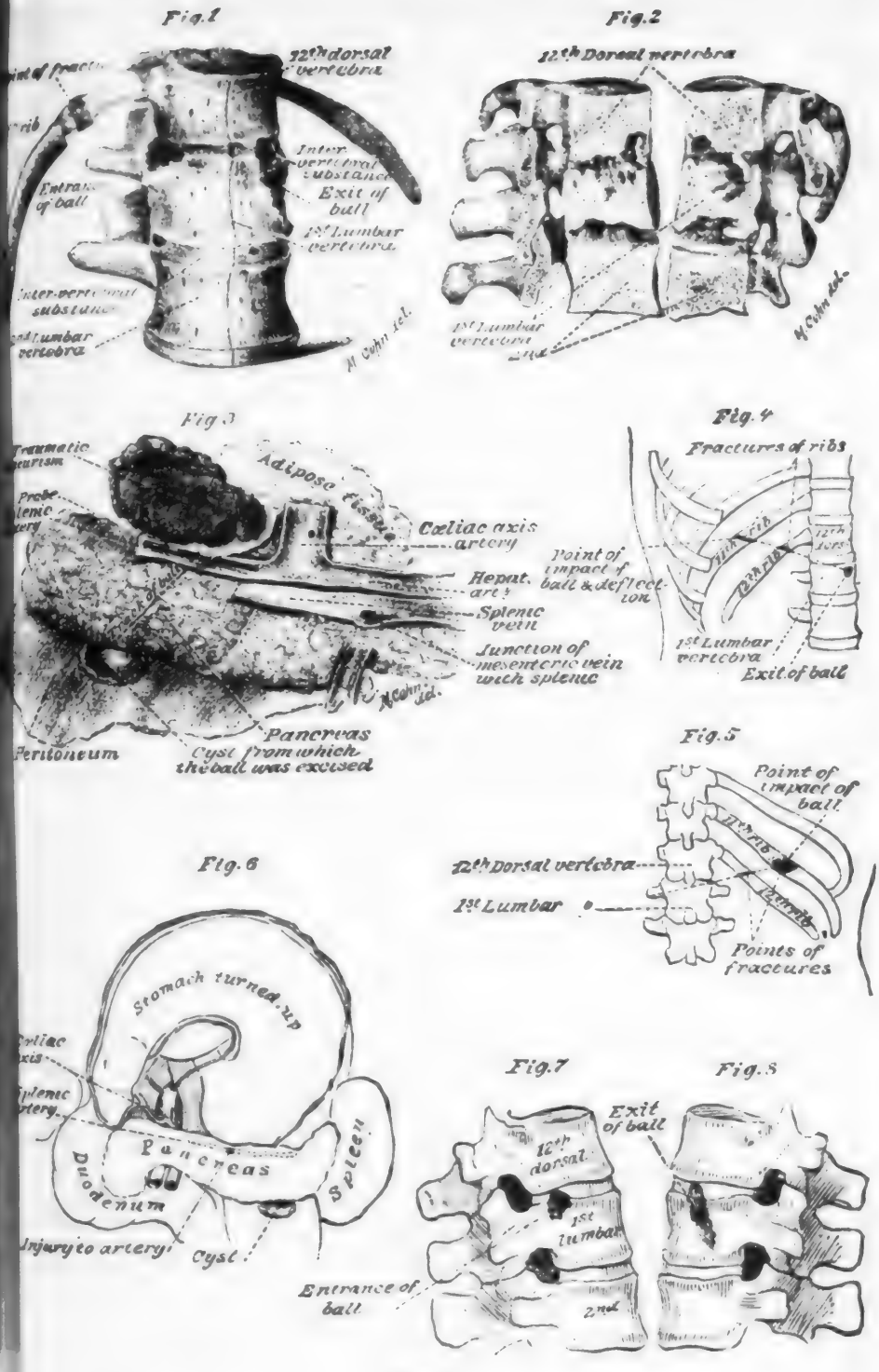

Figure 1

Drawings of pathological specimens of Garfield's autopsy. From the *Medical Record*, October 8, 1881.

and spleen, but these adhesions were explained as being due to "an attack of chronic dysentery from which the patient suffered during the Civil War." About a pint of bloody fluid was found in the abdomen. Dissection was continued and the pistol ball and its track were located. The ball had entered the body 3½ inches to the right of the vertebral spine, fractured the eleventh rib, and had "gone to the left, obliquely forward, passing through the body of the first lumbar vertebra (shattering most of it, but missing the spinal cord entirely) and had lodged in the adipose tissue just below the lower border of the pancreas, 2½ inches to the left of the spinal column and behind the peritoneum." The ball was completely encysted, and the track was well healed for an inch or so behind it. Thus, the ball itself had been completely harmless after it had reached its last resting place. It now became evident what had caused the President's sudden death, for lying behind the peritoneum, between the shattered first lumbar vertebra and the encysted bullet was a blood cavity which had been produced by a rent $\frac{1}{10}$ of an inch long in the splenic artery, about 2½ inches from the coeliac axis. This rent must have occurred several days before death, since the walls of the tear were adherent to the surrounding tissue, and there was organization of part of the clot. Thus, a slow leakage of blood explained the chills and weakness from which the President suffered a day or so before death, and when the artery finally gave way on the night of the nineteenth, the pressure of the blood burst through into the peritoneum, explaining the blood in the peritoneal cavity and the sudden pain and collapse just before the President died. The splenic artery had probably been damaged originally by the pistol ball or by the physicians' probes, but not sufficiently to cause serious hemorrhage. However, with the passage of time, and the associated infection and debilitation of the patient, the traumatic aneurysm that formed finally gave way, and caused death by an exsanguinating internal hemorrhage.

On the right side of the spinal column the track of the bullet was enormously dilated and filled with pus. This collection had also burrowed behind the right kidney, and down between the peritoneum and the right iliac fossa, into the right groin exactly as a cold abscess does in tuberculosis of the vertebra. "This long-descending channel now clearly seen to have been caused by the burrowing of pus from the wound was supposed during life to have been the track of the bullet," and this was the track into which Agnew passed the irrigating catheter with such ease. This massive collection of pus was the cause of the irregular toxic temperature which had been present for so long a time.

The first lumbar vertebra was extensively comminuted, and minute fragments had been driven into the surrounding tissues causing several small isolated abscess cavities.

The twelfth rib was fractured, a fact not recognized during life. The liver was not involved in any manner except that a small abscess, which showed no connection with the rest of the wound, had formed around the gall bladder. The liver, however, was enlarged, and was pale yellow from fatty degeneration, reflecting the poor nutritional status of the President. The spleen was large, soft and flabby, but showed no abscesses or infarcts. There was no evidence of peritonitis. The right kidney showed three small superficial cysts, while the left kidney showed the only evidence of metastatic infection—a small ⅓ inch abscess in its cortex. The heart was soft and flabby, and showed a few atheromatous placques in the aorta. The lungs showed pleural adhesions, bronchopneumonia with spotty hepatization, hypostatic congestion and pulmonary edema (partly due to the embalming fluid injected before autopsy). No other pathological findings were noted.

That night the surgeons made their official reports, gathered their belongings together, and went their separate ways. Thus ended the President's case.

Garfield's body was taken to Cleveland, Ohio, where he was buried on September 24, 1881. A few articles appeared pro and con concerning the management and final outcome of the case, but most authorities agreed that the President received the best possible care. Some surgeons maintained that the President could have been saved if there had been adequate debridement of the gunshot wound at once, with thorough antiseptic cleansing and removal of bone spicules. These men were undoubtedly correct, but they were small unknown voices speaking against the giants of the surgical profession of their day. However, one must not be too hasty, in condemning the surgeons for the manner in which they explored and treated Garfield's wound. It is quite probable that their failure to use strict antiseptic technic was not entirely resistance to Lister's theory, but rather due to a lack of full comprehension of what Listerism implied. For example, only three years before Garfield was wounded, Agnew wrote in his text, *The Principles and Practice of Surgery*, that after a trial of one year he had become convinced that " the antiseptic method of Prof. Lister for the treatment of wounds . . . possesses advantages over all others." But later on in the same volume speaking of penetrating wounds of the chest he said, " The wound should be examined by the fingers or probe,"

Figure 2

Photographs of Charles J. Guiteau taken by C. M. Bell on July 4, 1881 in Washington, D. C. From the *Boston Med. and Surg. Journal*, February 9, 1882.

and here as often elsewhere, he failed to emphasize the need for adequate disinfection.

Thus, Garfield's untimely death cannot be entirely blamed on any one person or act, but rather upon a combination of circumstances which appeared to be unavoidable.

Epilogue

Guiteau was held in prison throughout the months of Garfield's illness. After Garfield's death, Guiteau was tried in the Federal Courts, found guilty of murder, and sentenced to hang.

Guiteau undoubtedly was insane, but the exact nature of his psychosis was never determined. His hereditary background was very poor. His father was a religious fanatic, and his mother suffered a post-partum psychosis at the time of his birth. His sister was an epileptic, and several of his uncles and cousins were in institutions for the insane.

Guiteau had always led an unsettled life, attempting many projects and never completing any of them. He changed occupations at frequent intervals, and was generally weak in judgment and lacking in common sense. However, it is reported that he was fairly " normal " up to the age of 18. Then, while a college student at Ann Arbor, Michigan, he suddenly became preoccupied with religion, quit school, and never after seemed normal. He tried his hand at practicing law, newspaper publishing, public lecturing, theology, and politics. For several months preceding his attempt on the President's life, he had been in Washington seeking an appointment as " Minister to Paris " which was rather unusual, despite the fact that the infamous " spoils-system " was then in effect. However, he was completely unknown to Garfield and naturally was refused any position. This undoubtedly precipitated the assassination.

Guiteau had been married for a few years, but had not lived with his wife for long. He gave a history of syphilis and gonorrhea and his physical examination showed his pupils to be unequal in size, a fact which might easily suggest neurosyphilis. He also had expansive delusions and maintained he was in the employ of " Jesus Christ and Company, the strongest firm in the Universe."

A final diagnosis was never made in his case, the courts believing him a " shrewd scamp feigning insanity." The true diagnosis was probably a combination of paranoid schizophrenia and neurosyphilis.

Despite the protestations of many medical men who believed him insane and not responsible for his actions, Guiteau was removed to the Federal

Penitentiary in Washington, D. C., and was executed by hanging on Friday, June 30, 1882, closing forever a case unique in the annals of medical history.

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NOTES AND COMMENTS

SIMON NEWCOMB AND THE LOCATION OF PRESIDENT GARFIELD'S BULLET

AN EDITORIAL NOTE

OWSEI TEMKIN WITH THE ASSISTANCE OF JANET KOUDELKA

Some time ago, Professor Aurel Wintner of the Johns Hopkins University kindly sent us the transcript of a letter, the original of which is in the possession of the Library of the Johns Hopkins University. It occurred to us that this letter may be of some interest in connection with the article by Dr. Stewart A. Fish on "The Death of President Garfield" printed in this issue of the *Bulletin*, pp. 378-392. We therefore now publish the letter together with a few comments on its background. At the same time we wish to express our gratitude to Professor Wintner and to the Librarian of the Johns Hopkins University, Dr. Homer Halvorson, for their helpful interest.

NAUTICAL ALMANAC OFFICE
NAVY DEPARTMENT
WASHINGTON, D. C.

July 8th, 1881

My Dear Sir:

I wish to submit the following question to you respecting the possibility of locating the bullet which shot the President.

Is there any possibility of determining its position two inches inside the body by any electrical arrangement? The only method which has occurred to me is its retarding effect upon a rapidly revolving magnet in its vicinity. If a small magnet rotating 100 times per second would be so acted on by a ball at the distance of two inches in the axis of rotation as to produce the retardation of one revolution per second in $\frac{s}{2}$ of time I should consider success pretty certain while it would still be possible if $\frac{s}{5}$ were required to produce this retardation. I fear however that the action would not be equal to this but would be greatly interested to have your opinion and also to know whether any other method of attaining the object occurs to you.

I am annoyed to find that my suggestion which I mentioned to one of the surgeons as a mere theoretical possibility has got into the newspapers as if it were a practical plan.

Yours very truly
S. NEWCOMB

Professor H. A. Rowland
Breeze Lawn House
Hunter, Green Co.
N. Y.

When Newcomb wrote this letter, he was superintendent of the American Ephemeris and Nautical Almanac.[1] The addressee, Henry Augustus Rowland (1848-1901), since 1876 professor of physics at the newly established Johns Hopkins University with which Newcomb came into contact in the same year,[2] was no stranger to him. Together with J. J. Sylvester and W. E. Story, the two men had joined in 1876 in issuing a pospectus for the foundation of an *American Journal of Pure and Applied Mathematics*.[3] Besides, Rowland's early work on electromagnetism made him a proper authority to be consulted about the problem raised in the letter.

From Newcomb's autobiography we learn that his acquaintance with Garfield reached back to 1862 when, for a short time, the latter boarded in the same house in which Newcomb resided. The acquaintance had been renewed during Garfield's tenure as a member of Congress, and Newcomb had the highest esteem for Garfield as a person as well as a legislator.[4] As soon as the news reached him that the President had been shot, he had inquiries made at the White House and kept a record of developments in his diary. Under the date of July 6, he made the following entry:

This P. M. sought an interview with Dr. Woodward at the White House, to talk of an apparatus for locating the ball by its action in retarding a rapidly re-

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[1] See the article on Newcomb in the *Dictionary of American Biography*, vol. 13, New York, Charles Scribner's Sons, 1934, pp. 452-455.

[2] During 1876-1877 Newcomb gave 20 lectures on the history of astronomy as a nonresident lecturer before a broad audience. From 1883 till 1893 he was head of the department of mathematics. See John C. French, *A History of the University Founded by Johns Hopkins*, Baltimore, The Johns Hopkins Press, 1946, pp. 88 and 140.

[3] *Ibid.*, p. 50 ff.

[4] Simon Newcomb, *The Reminiscences of an Astronomer*, Boston and New York, Houghton, Mifflin and Company, The Riverside Press, Cambridge, 1903, p. 353 ff.

volving el. magnet. I hardly think the plan more than theoretically practical, owing to the minuteness of the action.[5]

It seems that Dr. Woodward mentioned Newcomb's idea to Dr. Bliss who referred to it in a newspaper interview. Accounts of this interview by a Washington reporter probably appeared in many newspapers. We have found in the Baltimore *Sun* of Friday, July 8, 1881, an article headed "What Dr. Bliss hopes and fears" which, under the date line of "Washington, July 7," quotes Dr. Bliss as saying:

> . . . Prof. Newcomb has made a suggestion, too, which possibly may help us to locate the ball; we want to do that. Prof. Newcomb is now in correspondence with scientific men at St. John's University [sic] to make the necessary apparatus. The theory is that by the use of a magnet and coil it may be possible to locate the bullet and to determine its depth from the surface. They have begun to make the instrument, and will first experiment by putting a bullet in the liver of an animal and wrapping it in hide. There seems to be good sense and practical science in the proposition and it may be successful.

Articles such as the above would explain the last paragraph in Newcomb's letter to Rowland. This letter, as well as the entry in his diary, make it clear that Newcomb himself felt by no means sure of the practical possibilities of his suggestion which envisaged the bullet acting as a metallic conductor which would retard the motion of the revolving magnet.[6] We do not know of Rowland's reply; whatever it may have been, the whole project soon took a different turn due to the active interest of Alexander Graham Bell.

At this point it may be mentioned that "electrical indicators" for locating bullets were known even before 1881. However, they consisted of probes which had to be introduced into the wound.[7] In the case of President Garfield the problem arose of locating the bullet by an apparatus that would not require interference with the wound. Newcomb's idea was the first to be made public in the newspapers. Another possibility suggested itself in the use of the induction balance perfected in 1879 by D. E. Hughes. A report appeared in the New York *Tribune* of July 11, 1881, of experiments made by George M. Hopkins with the induction balance for this purpose.[8] Quite independently, Bell too had been thinking

[5] *Ibid.*, p. 356.

[6] Newcomb, *Reminiscences*, p. 358.

[7] The third American edition of Thomas Bryant's *Manual for the Practice of Surgery*, Philadelphia, Henry C. Lea's Son and Co., 1881, p. 944, mentions two "electric indicators."

[8] Alexander Graham Bell, Upon the electrical experiments to determine the location of the bullet in the body of the late President Garfield: and upon a successful form of

of utilizing the principle of the induction balance with a telephonic device. During the winter of 1877-1878 he had conducted experiments, the method and results of which he described as follows:[9]

> My method was to pass a rapidly interrupted voltaic current through one flat spiral while I examined its field of induction by means of another flat spiral connected with a telephone. The currents induced in the latter coil produced a musical tone from the telephone.
>
> At every point in the field of induction it was found that by turning the plane of the exploring coil a position of silence could be obtained, and another of maximum sound, the two positions making a right angle with one another.
>
> It was also noticed that when a position of silence was established a piece of metal brought within the field of induction caused the telephone to sound.

While engaged upon experiments to adopt this or the original apparatus of Hughes to the case of President Garfield, Bell read of Newcomb's suggestion in the newspaper.[10] The two men communicated[11] and this led to the following result which we quote in Bell's own words:[12]

> At his [Newcomb's] suggestion I tested the point whether the rotation of a leaden disk and of a leaden bullet underneath a delicately suspended magnetic needle would cause a deflection of the needle.
>
> The disk occasioned a deflection, but the bullet produced no sensible effect. I telegraphed the result to Prof. Newcomb, and at the same time took occasion to inform him of the hopeful results I had obtained with the crudely constructed induction balance referred to above.
>
> I was much gratified by his immediate appreciation of the experiment. He telegraphed that he thought an induction balance promised a much more hopeful solution of the problem than his own method, and encouraged me in every way to continue my experiments.

Thus Newcomb's idea was abandoned and Bell, now also supplied with Mr. Hopkins' communications and a Hughes' induction balance forwarded by Mr. Hopkins to the White House,[13] took the lead. On July 14, at 5 p. m., the following Report to the Consulting Physicians, which we quote from the Baltimore *Sun* of July 15, was issued in Washington:

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induction balance for the painless detection of metallic masses in the human body, *The American Journal of Science*, 3. series, vol. 25, Jan.-June 1883, pp. 22-61; see p. 26.

[9] *Ibid.*, p. 23.

[10] *Ibid.*, pp. 24-26.

[11] According to Bell, *ibid.*, p. 26, it was Bell who "telegraphed to Prof. Newcomb the offer of [his] assistance in carrying on experiments, knowing the comparative difficulty he [i. e. Newcomb] would experience in having apparatus made in Washington." According to Newcomb, *Reminiscences*, p. 358, it was Newcomb who approached Bell because he believed him to be the man to succeed with the project.

[12] Bell, *loc. cit.*, p. 26.

[13] *Ibid.*, p. 26 f.

Prof. Graham Bell and Prof. Taintor [14] came here from Boston today for the purpose of making, under the supervision of the attending surgeons, a series of experiments intended to test the practicability of ascertaining by electrical means the location of the bullet which lies imbedded in the President's body. They were driven at once to the Executive Mansion and are now, together with Prof. Newcomb, in the surgeons' room. The instrument which will be used in these experiments is an extremely delicate one known as the induction balance. It will indicate the location of an iron or copper bullet which lies imbedded at a depth of four or five inches, but as it is not nearly so sensitive to the presence of lead there is yet some doubt as to its utility in the President's case. If the experiment should prove successful, the induction balance will soon be used to locate the ball in the President's wound.

Newcomb continued to encourage Bell who, moreover, was aided by many scientists, Rowland among them.[15] But in the main the work as well as the responsibility were left to Bell and Tainter. This was fortunate for Newcomb's reputation, for when after some postponement trials were actually made on the President, the results were neither conclusive [16] nor subsequently confirmed by the post-portem findings.[17] Actually Bell improved his device so that it was used in locating bullets. But this success came only after President Garfield's death,[18] after the episode in which Newcomb had figured had come to its tragic close.

[14] Sumner Tainter, a coworker of Bell's.

[15] Bell, *loc. cit.*, p. 28 ff. and 38. Rowland also advised Bell on July 25, the day preceding the first experiment on the President.

[16] *Ibid.*, p. 48 ff.

[17] Newcomb, *Reminiscences*, p. 358.

[18] Bell, *loc. cit.*, p. 54 ff., and Catherine Mackenzie, *Alexander Graham Bell*, Boston-New York, Houghton Mifflin Company, 1928, p. 240.

THE CIRCULAR MOTION OF THE BLOOD AND GIORDANO BRUNO'S PHILOSOPHY OF THE CIRCLE

WALTER PAGEL

The present author believes that the discovery of blood circulation has an ideological background in the view of the circle and circular motion as a basic cosmological pattern. If this is true, the works of Giordano Bruno (1548-1600) should form a fertile source for relevant allusions. For it is his cosmology that is dominated by the idea of the supremacy of the circle and circular motion. In fact, his treatise " De Rerum Principiis " of 1590, one of the genuine works of Bruno preserved in the Noroff-collection, contains the assertion of the " continual and rapid " movement of the blood, " in a circle." Bruno says: " The spiritual life force is effused from the heart into the whole of the body and (flows back) from the latter to the heart, as it were from the centre to the periphery and from the periphery to the centre following the pattern of a circle. . . . The material part of all these spirits is a fluid which cannot move on its own account, but only by means of its innate spirit. Hence there is no circular or spherical motion outside the living organism. For the *blood which in the animal body moves in a circle* in order to distribute its motor, the spirit, lies immovable outside the body, is inert and subject to decay, no longer deserving the name of blood " (*Opera Latina*, ed. Tocco et Vitelli III, pp. 507 ff., notably p. 521 ff., Florence, 1891). He also says (*ibidem*): " in us the *blood* and other fluids are being *moved continually and rapidly in a circle*, flow and flow back, are diffused from the centre into the extreme periphery and from there return to the centre," [1] a movement

[1] De rerum principiis, *Opera Latina*, ed. F. Tocco et H. Vitelli, vol. III, Florence, 1891, p. 521 ff. " . . . de spiritu, qui est et facit in singulis singula, loquimur et inquirimus, et tandem definimus ipsum esse substantiam per se mobilem, et motu suo vitam, vegetationem et consistentiam rebus animatis communicare. Ipse est per se vivens et alia per ipsum, ipse est vehiculum omnium virtutum . . . et hic tum est unus universi, tum est pro individuorum innumerabilium multitudine multiplex, sicut in singulis proprius est motus atque singularis, non ad unam sed ad omnes partes, sicut a *corde virtus, quam spiritalem appellant ad totum corpus vitalis effunditur, et a toto corpore ad cor, tanquam a centro ad circumferentiam et a circumferentia ad centrum sphaerae progressione facta.* Et haec est prima et praecipua spiritus significatio, qui ab anima differre non videtur. . . . Quorum omnium (namely volatile substances which are also called " spirits ") substantia est aqua seu humor, quorum non est per se moveri, sed per spiritum inexistentem in eis; cujus rei signum est quod extra corpus illum motum in circulum seu in sphaericum non admittunt. *Sanguis enim, qui in corpore animalis in circulum movetur, quia vectorem*

which has never been properly explained by the peripatetic schoolmen who invoke confused generalities such as an instinct of nature, the necessity of fate, divine providence, etc., instead of the reality of the vital force immanent in its material vector, the blood. There are two further allusions to the circular movement of the blood in two of the better known Latin treatises of Bruno, " De Monade " and " De Universo et Innumerabilibus." [2]

Bruno may thus be regarded as a link bridging the gap between Caesalpino (1571)—the first to use the term " circulation "—and Harvey (1615-1628), the discoverer of the final scientific synthesis of observational truth and cosmological postulate.

spiritum ibi sortitur, extra corpus jacet immotus, torpet, putrescit et non dicitur amplius sanguis, nisi aequivoce; similiter et aquae extra suum locum, extra fontes et extra flumina putrescunt. . . . ." (italics are mine).

[2] *De Monade, item de Innumerabilibus,* Frankfurt, Wechel, 1591, pp. 524-525: " Ut in nostro corpore sanguis et humores omnes virtute spiritus per totum *circumcursant et recursant,* sic in toto mundo, astro, Tellure; quidenim in nostro corpore dices deorsum? pedes? tibias? cur non illic omnia distant gravia? "

Ibid., p. 23: " Unum in microcosmo centrum est *cor, a quo* per totum animal spiritus vitales egrediuntur, in quo arbor universa vitae figitur et radicatur *et ad cujus* primitivam custodiam et conservationem *referuntur.*"

CORRESPONDENCE AND REPORTS

SUMMER MEETING AND PILGRIMAGE OF THE SCOTTISH SOCIETY OF THE HISTORY OF MEDICINE

The Scottish Society of the History of Medicine held its second annual summer meeting on Saturday, 24th June 1950, in the Royal Society of Edinburgh. The President, Dr. Douglas Guthrie, introduced Professor Richard Shryock of Baltimore, who gave an account of the events which led to the foundation of the Johns Hopkins University in that city. He traced the varying attitude towards medical education and practice in the United States during the 18th and 19th centuries, emphasizing the conflicts between utility and culture, and later, between administration and teaching. The difficulty of securing funds for the establishment of universities could not be solved until the captains of industry became convinced of the utility of those institutions. The purely cultural value of learning did not impress them. A second conflict, which still exists, arose from the fact that administrators ranked more highly than teachers, in salary and also in prestige. Founded with the primary object of promoting research and post-graduate study, the establishment of Johns Hopkins University was a brave effort to raise the cultural level and to encourage learning for its own sake and not merely with a view to material gain. The signal success of the medical school was largely due to the wise choice of the original professoriate.

Professor Whitfield Bell of Dickinson College, Carlisle, Pa., contributed the second paper, which dealt with the life and work of Dr. John Morgan who, after studying at Edinburgh and on the continent, returned to America full of honours in 1765, and established the first medical school, in the University of Pennsylvania.

During the afternoon following the meeting, the Society made a pilgrimage to several places of medical interest in the county of West Lothian, including the grave of William Cullen at Kirknewton, the early home of Robert Liston at Ecclesmachan, and the Perceptory of the Knights of St. John at Torphichen.

Dr. William Cullen, who founded the medical school of Glasgow and who later succeeded John Gregory as professor of medicine at Edinburgh, had his country residence near Kirknewton. In recent years his tomb was renovated by the Royal College of Physicians of Edinburgh.

Liston, the son of the parish minister of Ecclesmachan, attained great distinction as anatomist and surgeon. While assisting John Barclay in the teaching of anatomy at Edinburgh, he participated in various "resurrectionist" activities, and it is said that he was the original of Dr. Macfarlane in Robert Louis Stevenson's novel, "The Body Snatcher." As the present minister, Rev. R. L. Pinkerton, explained to the Society during the visit to the church, some evidence of Liston's work came to light in a curious fashion a few years ago, in the course of certain structural repairs to the roof of the manse. A young workman fell through the decayed timbers above the attic, and, to his surprise and horror, found himself literally in the arms of a skeleton. He fled from the scene and did not stop running until he

reached his home in Linlithgow: nor could he be persuaded to return to the task. The skeleton was afterwards decently interred in the churchyard, to the chagrin of certain members of the medical profession, who would have preferred to preserve this grisly relic of the work of the great anatomist.

The final objective of the pilgrims was Torphichen Priory, which is now scheduled as an Ancient Monument. Little of the original building remains, but the crossing and transepts, surmounted by a bell tower, still represent the Perceptory which, established in 1124 A. D., became the chief seat of the Knights Hospitallers of St. John in Scotland. In 1547 the building was transferred into the hands of the Government by the last perceptor, Sir James Sandilands, an ancestor of the present Lord Torphichen.

The Society, founded in 1948, exists for the promotion of interest in the history of medicine, and is neither exclusively Scottish nor exclusively medical. The Hon. Secretary is Dr. H. P. Tait, 137 Colinton Road, Edinburgh.

ANNOUNCEMENTS

AMERICAN ASSOCIATION OF THE HISTORY OF MEDICINE

Officers elected

The following officers were elected at the twenty-third annual meeting of the Association in Boston, May 21-23: President, Dr. Ralph H. Major; Vice-President, Dr. John F. Fulton; Secretary, Dr. Iago Galdston; Treasurer, Dr. Emerson C. Kelly; Editor, Dr. Owsei Temkin.

Awards of medals

At its twenty-third annual meeting in Boston, May 21-23, the American Association of the History of Medicine, upon recommendation of a committee consisting of Dr. Iago Galdston, chairman, Dr. Owsei Temkin, and Dr. E. B. Krumbhaar, made the following awards:

The William Osler Medal for the best student essay submitted was given to Thomas Franklin Williams, a fourth-year student at Harvard Medical School, for an essay on "Cabot, Peabody, and the Care of the Patient."

The William H. Welch Medal "for particular contributions of outstanding scholarly merit in the field of medical history" was given to Dr. Henry E. Sigerist, formerly William H. Welch Professor of the History of Medicine at the Johns Hopkins University. In its recommendation the committee included the following citation of Dr. Welch on Dr. Sigerist: "Combining in equal degree complete command of the history of medicine with full knowledge of the present state of the science and art of medicine and with penetrating insight into the past and present social, cultural and philosophical bearings of the doctrines and practices of the healing art," Henry Sigerist's life work "illustrates not merely the interest and cultural value of historical studies and the advantages of the historical method of exposition, but also the absolute necessity of such studies for an adequate

understanding and interpretation of the present state and outlook of medical knowledge and practice."

The full reports of the committee on both awards will appear in the transactions of the meeting of the Council.

Committee on the Teaching of Medical History

At the annual meeting of the Council of the Association held May 21, 1950, it was voted that a committee be appointed to consider the teaching of medical history and make recommendations to the Association at the next meeting. Accordingly, Dr. Viets, President of the Association, has appointed such a committee. Dr. Shryock will serve as chairman and will make arrangements for a meeting of the committee in Baltimore some time in October or November.

NATIONAL NEWS

Army Medical Library

In an article on "The Army Medical Library's Publication Program" (*Texas Reports on Biology and Medicine*, 1950, vol. 8, pp. 271-300), Major Frank B. Rogers, Director of the Library, and Mr. Scott Adams, Assistant to the Director, trace the development that has led to the discontinuance of the Index Catalogue (subsequent to publication of Vol. XI, Mh-Mz) and its substitution by two publication series: The *Army Medical Library Author Catalogue* for monographs, and the *Current List of Medical Literature* for articles in medical journals. The authors make it clear that the Index Catalogue, if continued along its traditional line, would meet with well-nigh insuperable difficulties and would fail to keep the medical world abreast of current publications. This latter aim, which is emphasized above all others, is more likely to be adequately met by the monthly publication of the *Current List* (with a cumulated author and subject index at the close of the yearly volume) and the yearly *Author Catalogue*. It is to be hoped that by a wide coverage of historical monographs and articles, medical history will also profit by this new venture. In view of the cessation of the *Mitteilungen zur Geschichte der Medizin, der Naturwissenschaften und der Technik* this seems especially desirable. On the other hand, the medical historian who, more than any other medical worker, is dependent on older literature will lose a practical tool in the Index Catalogue. While at present he has to consult its four series, he will, in let us say ten years time, have to consult an additional twenty volumes. One may wonder whether this will not eventually lead to the creation of a catalogue for the purpose of analyzing the indices critically so as to separate the wheat from the chaff.

O. T.

Baltimore

Dr. Harry Friedenwald died on April 8, 1950, at his home in Baltimore. The *Bulletin* plans to publish an obituary of Dr. Friedenwald in a later issue.

Philadelphia

Bequests, totalling $20,000, from the estates of William Edward Mead and his wife, the late Dr. Kate Campbell Hurd-Mead, have been given to the Woman's

Medical College of Philadelphia, "the income to be used toward a lectureship on the history of medicine to be known as the Kate C. Hurd '88 Fund." (*J. A. M. A.*, June 17, 1950, vol. 143, no. 7, p. 664)

NEWS FROM ABROAD

Edinburgh

For purposes of illustrating a lecture on "Simple Herbs and Synthetic Remedies" in Dr. Douglas Guthrie's summer class of the History of Medicine (see this *Bulletin*, 1950, vol. 24, p. 292 f.), a number of books and manuscripts were exhibited during May and June 1950. In order to give the exhibit more permanent value, Dr. Guthrie has brought out a booklet, *Some Early Herbals and Pharmacopoeias* (Edinburgh, 1950) which links the pieces on exhibit by a short narrative account amplified by illustrations.

London

An exhibition at the Wellcome Historical Medical Museum illustrating *Medicine in 1850* was formally opened on Tuesday, May 30, 1950. An illustrated *Catalogue* of this exhibition has been published for the Trustees of the Museum by Geoffrey Cumberlege, Oxford University Press; price $.60.

Meetings

The medico historical section of the Svenska Läkaresällskapets announced a meeting for May 10, 1950.—A meeting was announced for May 24 by the Instituto Brasileiro de História da Medicina in Rio de Janeiro.

BOOK REVIEWS

HENRY ALAN SKINNER. *The Origin of Medical Terms.* Baltimore: The Williams and Wilkins Co., 1949. viii + 379 pp. $7.00.

A reference work of standard medical terms, their derivation, their meaning and their history. It has been designed chiefly for the needs of first-year students. It also contains short biographies. These include not only those who have lent their names to medical nomenclature and who are in any way connected with medical terminology, but medical men and chemists of note of all times.

The author, Professor of Anatomy in the University of Western Ontario, has certainly achieved the purpose he has set himself and must be congratulated on a work incorporating a labour of love through many years. The book can be characterised as a happy solution of a difficult and urgent problem—a solution which bespeaks a high educational standard and a fine understanding for the needs of the student and indeed of anyone interested in the elements of medical terminology. The vastness of the material necessitates a compromise and it would be preposterous to criticise the author's choice, and the emphasis he wishes to convey, in such a matter. A few points, however, may be discussed. There is an understandable preference given to anatomy and chemistry. Harvey, for example, has 21 lines, while anatomical nomenclators such as Albinus and Waldeyer have 35 and 22 lines respectively. There is nothing about "Vitalism," "Mechanism" and "Animism," but Rud. Wagner has been accorded 20 lines. We hear nothing of, say, Hyrtl, Tiedemann, Du Bois Reymond, Rosenthal, Hermann, Bernstein, Samuel, Rubner, Hertwig, Beaumont, Spallanzani, Redi, Bonome, Cestoni, Czermak—as against, for example, Berengar Carpi, Geber, Basile Valentine, Arnald of Villanova, Roger Bacon, Sauvages, Gerard of Cremona, Burdach and C. B. Reichert who are represented. There is an article on Galileo, but not on Kepler whose fundamental work on the physiology of vision would justify his inclusion. Cesalpino appears as the teacher of Galileo and as a forerunner of Harvey (under "circulation") but could well claim a biographical note of his own. So could Colombo who does not seem to occur at all. There are good biographies of Addison, Bright, Rokitansky, Virchow, Cohnheim etc., but it is said of Karl Weigert that "the one (sc. stain) which is known to-day as Weigert's method is his myelin sheath stain . . ."—a surprising statement. It is not without irony to see the founder of histochemistry, Paul Gerson Unna, presented as the Heros eponymos of a skin paste and nothing else, in a book chiefly meant for pre-clinical education. Vital staining is attributed to Ehrlich (1881)—Ponfick's and Waldenburg's efforts which go back to 1869 might have been mentioned. We missed "hemosiderin" and "lipofuscin," the "rhesus-factor," "immuno-chemistry," "histiocytes," "plasma-cells," "Langhans giant cells," "Greenfield-Sternberg-cells" etc. In the isolation of Penicillin the work of the biochemist and Nobel-prize winner, Chain, should not remain unmentioned. There are very few references to pathological syndromes and their describers. For example, Cushing's elucidation of pituitary physiology is briefly hinted at, but "Cushing's disease" fails to appear. There are a few incon-

sistencies in general medical-historical evaluations. A good short article on Van Helmont credits the latter with the criticism of the catarrh-dogma, but under "catarrh" the whole credit for its abolition goes to Schneider. The only source given for Egyptian medicine is the Ebers Papyrus. The appended bibliography is—with some notable exceptions—a depressing list of secondary and tertiary sources. It reflects the tendency to direct the student to popular books which even the freshman should be able to understand. The reviewer feels that the student is better left without a book than with an unsatisfactory one. On the other hand, there are books which the student would and could peruse—anima salva—, notably Withington's *Medical History from the Earliest Times.* It is true that it has been out of print for some long time.

None of these points, however, can detract from the distinct value and the notable amenities of the present book in which the reviewer failed to see any inaccuracies (apart from a few printer's errors). It is full of interesting, valuable and lucidly presented information.

WALTER PAGEL.

RUDOLPH KAYSER. *The Life and Time of Jehudah Halevi.* Translated from the German by Frank Gaynor. New York: Philosophical Library, 1949. 176 pp. $3.75.

It must have been books of this kind that the Preacher had in mind when he declared "of making many books there is no end." About the best thing that could be said concerning it is that the author was honest when he confessed in his introduction that he was not presenting any new material to his readers. Had he been able to overcome the urge to break out into print, he would have spared himself a lot of unnecessary effort and trouble. For so far as the story of the life of Jehudah Halevi is concerned, it has been told far more beautifully in the Schiff Library of Jewish Classics (Philadelphia 1928) by Heinrich Brody. As for an appraisal of the poetry of the Minnesinger of Zion, that, too, has been done with much more discrimination in the little volume of the selected poems of Jehudah Halevi issued by the Jewish Publication Society of America than in the monograph by Rudolph Kayser. He, again, who is interested in the philosophy of Judaism propounded by Jehudah Halevi's *Kusari,* will find a much more satisfactory outline in Isaac Husik's *History of Medieval Jewish Philosophy* (Philadelphia 1930) than in the book under discussion.

The only original idea contributed by the author is that which assigns reason to the West and religious mysticism to the East, which is, to put it mildly, a rather daring generalization. How he arrived at this as well as other unwarranted conclusions, is a bit difficult for the critical student to figure out. The book is full of such inaccuracies as "philosophic research . . . resurrected the Hebrew language" (p. 48), "Jehudah Halevi, this absolutely unorthodox Jew, opposed to petrified rabbinism in every form" (p. 112), and "He (i. e. Maimonides) . . . knew also the heavenly passion of the poems of Jehudah Halevi." (p. 155). Anyone acquainted with the Jewish philosophical literature of the Middle Ages knows that

it was not until the middle of the twelfth century that the classics of Jewish philosophy were translated from the Arabic original into a very clumsy and unidiomatic Hebrew, and that at that time the golden age of Hebrew letters in Spain had already passed its zenith. To describe as an opponent of orthodox Judaism a man who defended all the minutiae of Jewish tradition and even put the ritual laws of the Pentateuch above its social precepts betrays ignorance either of the contents of the *Kusari* or of the meaning of the term "orthodox." Finally, how one can speak of that prosaic rationalist, Moses Maimonides, as possessing the passion of the poems of Jehudah Halevi is beyond this reviewer. But why wonder at such statements by a writer who declares that "in the North, Jews lived in Ghettos, closed in by the lifeless walls of rationalistic Talmudistics" (p. 67), when it is known that it was first in the sixteenth century that the ghetto was officially and formally established in Italy?

Among the many questions one is tempted to ask of Mr. Kayser is the reason for the title of the book. Why did he call it "The Life and Time of Jehudah Halevi" when it begins with the Moslem invasion of Spain, which took place more than three centuries before the birth of Jehudah Halevi, to whose career only four of the eleven chapters of the book, comprising 64 out of a total of 170 pages of text, are devoted? And what prompted the author to include a summary of Moses Maimonides' *Guide of the Perplexed* in a monograph dedicated to the life and time of a poet-philosopher who was already dead when the sage who was to write the "Guide" was still a child?

The deficiencies from which this little volume suffers may still not very seriously have impaired its use for popular consumption were it not for the many misspellings, such as "Abassides" (p. 12), "Montenebbi" (p. 24) and "Mishnah Torah" (p. 157), and the very poor translation. The book just bristles with sins against English syntax, prosody and idiom, which it would be too wearisome to enumerate. Expressions like "split allegiances" (p. 92) and "obeyance of the Laws" (p. 146) give the impression that the translator had gotten hold of a German-English dictionary and either made use of the first synonym he found or coined his own noun from the verbal root given. In short *The Life and Time of Jehudah Halevi* by Rudolf Kayser is very much in need of a thorough overhauling in order that it might serve the purposes for which it was intended.

SAMUEL ROSENBLATT

MAURICE BEAR GORDON. *Aesculapius Comes to the Colonies.* The Story of the Early Days of Medicine in the Thirteen Original Colonies. Ventnor, N. J.: Ventnor Publishers, Inc., 1949. xiv + 560 pp. Ill. $10.00.

This book is a compilation of general and medical historical information, largely derived from reference works (*Encyclopedia Britannica*, Packard's *History of Medicine in the United States*), authoritative regional medical histories and recent medico-historical papers. The publication of such a volume is timely, despite the defects apparent in its substance.

The text is developed upon a rigid framework, allowing one chapter for each

of the thirteen colonies. Every chapter opens with a statement of the general history of the colony in the seventeenth and eighteenth centuries. Then follows a procession of brief, outline biographies of numerous colonial physicians, moving in conventional style from birthday to death, mentioning, whenever possible, the probable cause of demise. Institutions and medical societies receive scanty attention, despite their social significance and more obvious continuity. Extremely limited accounts are given of influential laymen, and one looks in vain for recognition of the works of William Byrd, F. R. S., and Thomas Jefferson, whose breadth of intellect transcended the narrow medical dogma of their day. Page upon page is devoted to isolated and limited practitioners and to the reproduction, verbatim, of writings, obscure, but representative of their age. The repetitious mode of presentation, plus scanty correlation of material, give little momentum to the account and produce marked similarities between chapters. Striking errors—"sins of commission" or typographical—abound, affecting general historical statements as well as those pertaining to medical affairs. The author makes no attempt to present a summation of significant changes in medical thought, education or practice in the two centuries encompassed. Indeed, commentary is exceedingly limited. The book is profusely illustrated with portraits and photostats of numerous documents and title pages, some of which are poorly reproduced, yet presenting a remarkable gallery of colonial physicians.

Doctor Gordon has devoted a great deal of time and interest to the compilation of a colonial American medical history. The work can lay no claim to originality and is frankly derived, with acknowledgments, from secondary sources. Criticism may well be expected from professional historians for minor infractions of scholarly method and careless errors. The book, it must be remembered, is intended for those interested members of the medical profession and for laymen, not so intent upon complete accuracy, who may find here a record, albeit incomplete and sketchy, of early America's medical pioneers.

ROBERT J. FAULCONER

The District Nursing Association, Northern Westchester County 1898-1948. Compiled by Katharine S. Oliver, Ed. [Mount Kisco, N. Y., 1948], xiv + 101 pp. Ill.

The major purpose of this history of the District Nursing Association of Northern Westchester County is to stimulate self analysis among its members, so that they might gain a fresh impetus to blaze new trails in rural nursing, under voluntary auspices. The book is a compilation of the material in annual reports of the parent agency and its local committees; extracts from an account of the Association, presented by Delia West Marble—an officer and board member for fifty years; personal communications and reminiscences by pioneering nurses and others; and a summary of the development of professional nursing during the years 1898-1948 by the present public health nursing director, Miss Virginia Lewis. This informal, vivid story has the quality and warmth of a "word of mouth" account about the courageous women who saw the desperate health needs of their neighbors,

vigorously attacked the problems, and out of vicissitudes and triumphs altered the "structure and tone of their community." The details of the founding of this first rural district nursing association, its steady growth, its ever-widening influence, and the establishment of its local committees are colorful and interesting. However, of wider significance is the tracing (in a local situation) of the gradual development of new concepts of rural nursing services, and preventive health measures, the expansion of the official county health department, and the steps in cooperative planning between that agency and the voluntary District Nursing Association. The account closes with a reiteration of the principles on which voluntary health agencies must function today and tomorrow. Emphasis is placed upon the quality of membership in such agencies; the importance of discovering and initiating new services as new community needs develop; of relinquishing appropriate activities to the official health agency; of recognizing and supporting the primary position of leadership of the official agency; and of submitting to periodic searching self analysis.

This book should be of interest to all concerned with the history of nursing and the development of community nursing services. It might well be added to reading lists for public health nursing students. Such accounts as this humanize, and make "three dimensional," sterile textbook facts.

GLADYS L. CRAIN

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